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PHILOSOPHICAL  
TRANSACTIONS,  
OF THE  
ROYAL SOCIETY  
OF  
L O N D O N.

VOL. LXXII. For the Year 1782.

PART I.

L O N D O N,

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## A D V E R T I S E M E N T.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought adviseable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.



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# PHILOSOPHICAL TRANSACTIONS.

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- I. *Relazione di una nuova Pioggia, scritta dal Conte de Gioeni  
abitante della 3<sup>a</sup> Reggione dell' Etna; communicated by Sir Wil-  
liam Hamilton, K. B. F. R. S.*

Read November 8, 1781 \*.

*Volat per Mare magnum cinis decoctus, et terrenis nubibus  
excitatis, transmarinas quoque provincias pulvereis guttis  
implevit.* CASSIOD. lib. IV. var. epist. 50.

**L**A mattina de 24 corrente si e qui presentato uno de feno-  
meni piu' singolari; tutti li luoghi esposti all' aria si tro-  
varono bagnati da un' acqua colorita cretacea biggia, la quale

\* For a translation of this paper see the Appendix.

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evā

evaporando indi, o infiltrandosi nella terra, lasciò per ogni dove la materia che contenea, all' altezza di due, o tre linee; tutti i ferri, che ne furono tocchi, divennero rugginosi.

Il publico portato al maraviglioso, imaginò varie cagioni di tale pioggia, motivi di timore per gli vegetabili, e gli animali.

Le popolazioni, che si valgono delle acque piovane, si assennero di farne allora uso. Vi fu' chi sospetasse contenersi in essa de' principi vitriolici: e delle persone predissero un qualche male epidemico sopravveniente.

Chi avea osservato l'esplosioni, che l'Etna, da venti e più' giorni, facea vedere dall' alto del suo cratere, inclinava a credere originato quel fenomeno da una di esse.

Si estese la pioggia dal nord  $\frac{1}{2}$  nord est al sud  $\frac{1}{2}$  sud-ovest sopra le campagne di noto, fin dove contansi in linea recta, settanta miglia, dal vertice dell' Etna.

Non è nuovo, che li vulcani per la forza espansiva, che violenta in essi si genera, abbiano cacciato delle Sabbie \* portate da venti di lontane reggione, e delle pietre †.

\* L'autorità di Cassiodoro, premessa a questa relazione e avvalorata da Seneca nel suo 2° lib. delle Quest. Nat.

*Ætna aliquando multo igne abundavit, ingentem vim arenæ urentis effudit, involutus est dies pulvere, populosque subita nox terruit.*

Ma senza riandare alle moltissime memorie di questo vulcano, e del Vesuvio, come a noi più' vicini, abbiamo da 20 anni in qua veduto molte delle piogge in Sicilia, originate dell' Etna, e l'ultima precedette la irruzione dell' anno scorso, era quella composta di piccioli frammenti di pomici bituminose, o stumie.

† La pietra, descritta da PLINIO caduta nella Tracia, la pioggia di pietra, che avvenne sul monte Albano, dopo la rovina di Alba, della quale ci fa menzione TITO LIVIO, e molte altre simili, rimarcate dalli antichi, come piogge prodigiose, sono state riconosciute per vulcaniche; in quanto all' Etna abbiamo a giorni nostri veduto formare de' monti nuovi, per il cumulo delle pietre, o per meglio dire delle lave; e degli antichi, oltre STRABONE, ed'altri molti scrittori, il lirico PINDARO ci trasmette, che, *aliquando non tantum rivus igneus ejecit, sed saxa ignita.* PIND. ap Brit. lib. V. c. 14. p. 2.

Ma

Ma il colore della materia, e la sua sottigliezza, diedero motivi di dubitare, onde ne fosse originata, accrescendosene l'incertezza dalla rimarchevole circostanza dell' acqua, che portolla incorporata \* e però si sospettava di altro principio.

Era dunque vopo per ogni ragione di assicurarsi della natura di tale materia, onde restar persuasi della sua origine, e degli effetti, che avrebbe potuto cagionare: non potea questo farsi senza il soccorso dell' analisi chimica: per far dunque ciò con sicurezza, procurai raccogliere quella pioggia in luogo, ove potessi credere, che non esistessero altre sostanze eterogenee: Scelsi perciò la pianta chiamata *Brassica Capitata*, la quale avendo le foglie larghe, e ravvolte, trovai, che contenea a sufficienza dell' acqua colorita; e riversate molte di esse in un vaso, lasciai indi, che spontaneamente ne risedesse la terra al fondo, in cui dopo qualche tempo depose la parte limosa, restando l'acqua trasparente.

Separata questa in altro vaso, la tentai con de' liquori alcalini vegetabili, ed acidi minerali: ma non osservai decomposizione con alcuno de' due mestruj; passai ad evaporarle per riunire quelle materie, che potevano forse essere in soluzione, e toccatala di belnuovo con gli anzidetti liquori, fece vedersi leggiera effervescenza con gli acidi; provata con lo siroppo di viole, divenne questo verde smorto, così che mi persuasi, che contenesse un sale calcareo †. Con la decozione di galla non produsse precipitazione.

\* Ne' molti scrittori del nostro Etna, non trovasi Pioggia di Sabbia, o di altra produzione mescolata con acqua.

† Provata ancora con dissoluzione di piombo nell' acido vegetale, perdette il suo color naturale, e la sua trasparenza, divenendo lattiginosa. Io mi farei a credere, che sù quello un' effetto delle particelle alcaline, e così spiegare la efflorescenza, che mosse sopra li ferri esposti all' aria.



Disseccata, poscia all' ombra la materia, si fe' vedere una terra sottilissima di color cretaceo, ma inerte per essere stata diluta della pioggia.

Penfai calcinarla ad un fuoco leggiero, e vi prese il color di mattone; posta indi in un crogiuolo, una porzione di questa la passai a fuoco piu' violento, e perdette quasi il colore acquistato; un'altra parte di questa provata a piu' gagliarda, e lunga calcinazione (onde potessi sperarne la vitrificazione) resto cio non pertanto, frolle, e divisissima, ritornando al suo pristino colore biggio.

Osservato con accuratezza il fumo delle tre calcinazioni, non diede alcuno colore, ne odore, per sospettarsi de' mescolanza arsenicale o sulfurea.

Avuta dunque quella materia in tre porzioni, calcinata a tre differenti gradi di fuoco, le presentai una buona magnete, ma non aggi questa nella prima, e nella seconda; nella terza pero' una leggiera attrazione, in aghi visibili, reiterata piu' fiate, mi fe' stabilire, che sia in questa terra un principio marziale nella forma metallica, e non in sostanza vitriolica\*.

Della natura dunque delle materie riconosciute, si rileva la loro origine volcanica, imperocche il ferro piu' che e' esposto a violenta calcinazione, perdendo il principio flogistico, piu' si rende diviso, e non puo questo succedere naturalmente, che nel gran focolare di un volcano. Il sale calcareo essendo un sale marino, combinato con sostanze calcaree per via di fuoco violento † non puo' altrimenti essere composto, che nel volcano ‡.

Percio,

\* Imperochè non avrebbe altrimenti l'acqua prodotto effervescenza con gli acidi, ma l'avrebbe mostrato con gli alkali, e nella triplicata calcinazione, si sarà piu' tosto accresciuto, che diminuito il colore rosso.

† La combustione delle pietre da Calce puo' produrre, e' vero, la combinazione,

Pertanto, che appartiene agli temuti effetti, sopra gli animali, e gli vegetabili, e noto a chiunque, l'uso vantaggioso che ritira la medicina dell' uno e l'altro, in quella stessa forma che furono preparati nel gran laboratorio della natura.

Li vegetabili, che sono nell' attuale horificazione, non mostrano la menoma macerazione, come altre volte e avvenuto con le piogge di Sabbia\*.

Come poi quella produzione volcanica si sia mescolata all' acqua, può cio concepirsi in varie guise.

L'Etna e ordinariamente attornato nella sua media regione di nuvole, le quali non sempre oltrepassano la sua sommità, che si alza a 2900 passi † sopra il livello del mare, cacciatane fuori quella materia, trovando sottoposte le nuvole, poté avvenire, che si fosse mescolata alle stesse, e sciolta poi in pioggia nella maniera ordinaria: può altrimenti conghietturarsi, che quel denso fumo, che contenea la materia volcanica per la forza de'

zione, onde risulso il sale calcareo, ma scorgesi chiaramente, che non potea quella quantità altronde provenire che dal volcano.

‡ Molte, e replicate esperienze sopra li prodotti dell' Etna mi hanno persuaso, che il sal marino si uno de principali e piu' abbondanti mestri, che eccitano le effervescenze del nostro volcano, o che ne si la base (come un amico di molta cognizione mi ha fatto nuovamente riflettere) trovo del sale calcareo nelle vecchie lave, del sale comune, lo trovo sublimato in ammoniaca nelle fenditure, e ne' spiragli delle nuove eruzioni; ma qui non e luogo a cio' che richiede un maggior volume, forse appresso potro' meglio dirne in altra occasione.

\* Mi trovo aver replicatamente osservato, che le piogge di Sabbia del nostro monte per lo piu' composte di materie calcinate, e di piccioli cristalli di Schorl, portano un cimento di particelle arsenicali, e sulfuree, e qualche volta saline, che unisce lo Schorl alle altre materie; cosiche se ne ingrossano li granelli; qualche volta ancora ci e arrivata la pioggia calda a terra.

† La misura, che ho tentato della perpendicolare del monte, mi e due volte riuscita all' altezza descritta, non pero la do per certa, sapendo che l'altimetria ha vopo di esatti istrumenti, e di reiterate osservazioni, che dovro comprovare ancora con il barometro a miglior comodo.

venti, fosse trasportato nell' atmosfera, con la sua rarefazione, sopra quel tratto di paese \*, e quindi raffreddandosi si sia condensato tanto, che superando il peso dell' aria sottoposta, si abbia sciolto nella pioggia colorita.

Io rimetto per altro a fisici, a quali appartiene la cognizione degli aggenti della natura, lo esame, e la spiega di tale fenomeno, limitandomi alle osservazioni, ed alle esperienze di naturalista iniziato nella chimica, affine di concorrere con esse di qualunque merito siano, alla teoria de' vulcani, e del globo †.

P. S. A 4 Maggio, Venerdì alle ore 21½ di Italia si è fatta sentire una scossa di terra assai leggiera, nelle abitazioni che sono attorno all' Etna, la quale piu' si rese sensibile in qualche lontananza del monte, la sua azione fu dal nord al sud. Avea il vulcano continuato le fiamme, e le esplosioni, e la notte precedente, una colonna di fumo, composta di globi quasi articolati, l'uno sopra l'altro, si era alzata sopra il cratere all' altezza duplicata della montagna per quanto facea arbitrare la distanza di

\* Accio la addotta ipotesi non sembri esagerata per la quantita di fumo, che devevi supporre, io rapporto cio che fu osservato da CICERONE, cratere flamma erumpit, fumo mixta tam copioso, ut dum Boreas spirat Melitam usque per aerà illum sublimem propellat ad ix. millia passuum spatium. CIC. de Nat. Deor. lib. II.

† La Physique (dice il Sr. WALERIO nella sua mineral. t. 2. Hidrol. 2. f.) est plus universelle dans ses vûes, et plus philosophique dans son examen, le physicien envisage, raisonne, explique, le naturaliste regarde, ramasse, et range; celui-ci vous dira il existe tel corps dans la nature, il est fait, soit au dedans, soit au dehors de telle ou telle maniere, il est de tel ou tel regne, classe, ordre, espèce, variété; celui la pretendra vous expliquer les causes de son existence, de ses formes, et de ses propriétés.

Appresso al Sr. WALERIO l'illustre Sr. LINNÉ nell' anal. transalp. anno 1740, ff. 2. cosi scrive; Physica est scientia de qualitatibus elementorum. Historia naturalis autem circa cognitionem corporum naturalium versatur: il vero naturalista dev' essere istruito della fisica, e della chimica ancora, ma non conosciamo ancor noi qui la divisione delle due scienze.

22 miglia dal vertice per linea retta, in cui è questa città; durò quella tutto la notte perpendicolare, folche si avea staccato uno de' globi, ed allungato all' ovest della sua cima; tratto tratto tutto l'interno della colonna, e della lingua prolungata di fumo venivano internamente illuminate da fuoco elettrico, che traspariva rosso cupo, estinguendosi gradatamente dal basso all' alto, in due secondi.

Ha continuato il fuoco sul cratere fin' oggi 8 Maggio, rigettando delle moli infuocate, le quali, vagamente rotolando giù per il cono, hanno illuminato quella regione, e si è versata della lava dal cratere in qualche quantità verso l'ovest nord-ovest; ma non ha avuto questa la forza di rompere li fianchi, o le pareti del vulcano, a tal che siamo nel caso di appropriarci quella memoria storica. MARCO ÆMILIO C. AURELIO Coss. *Ætna mons terræmotu, ignes super verticem late diffudit.* Jul. Obsequ. de Prodig. c. 89.

II, *Nova experimenta Chemica quæ ad penitiorem Acidi e Pinguedine eruti cognitionem valere videntur.* Scribebat D. Laurentius Crellius, Gulielmo Huntero, M. D. S. R. S.

Read November 15, 1781.

**N**ON sine maxima animi voluptate e Philosophicarum Transactionum volumine novissimo percepi, meas litteras ad te missas, de experimentis chemicis referentes, non solum tibi haud displicuisse; sed etiam illustrissimæ Regiæ Scientiarum Societati abs te, ea qua es humanitate oblatas, honorificentissimis illius suffragiis esse ornatas. Quanti hunc insignem in me collatum honorem faciam, quanta sit mea in celeberrimos viros, summa benignitate de commentariolo meo qualicunque judicantes, reverentia, quantæ denique grates, quas tibi, vir celeberrime, debeam, non possum verbis fatis demonstrare. Persuasum itaque te habeas, oro rogoque, de mente mea tibi devinctissima; quam ut quoque, omni qua polleo, facultate tester, ill. Reg. Soc. subjunxi huic epistolæ nova ea experimenta chemica, quæ ad penitiorem acidi e pinguedine eruti cognitionem valere mihi videbantur; ea spe fretus, fore, ut hujusce disquisitionis chemicæ partem posteriorem judicans non inferiorem priori, humanissime illam offeras meo nomine Sociis celeberrimis, in summæ meæ in illos reverentiæ documentum.

Proposui ut nosti, vir celeberrime, modum istum, acidum pinguedinis concentratum acidi vitriolici ope obtinendi, quod nimirum, affusum sali segneriano ex hac partem acidam expellit

expellit forma vaporum. Quæ autem ne obnoxia sit objectioni, quod huic vitrioli oleum sit admixtum, et ut cognoscerem, quomodo se habeat sal medium nostrum, igne ustum, huic illud tradidi.

E X P. LVI.

Tres nimirum salis segneriani (seu ex nostro, e pinguedine destillata eruto acido, et sale alcalino vegetabili conflati) uncias, retortæ vitreæ loricatæ inditas, igni aperto per gradus aucto, exposui. Destillabat in initio in excipulum aquosi quid (crystallisationis nimirum aqua). Increcente calore, ita ut retorta ignire inciperet, protinus surgebant vapores copiosissimi grisei, acidum forte, ut mihi quidem videbatur, præfagientes: sed vasibus frigefactis, et apertis nihil fumi percipiebam, nec odorem acidi suetum; sed potius illum spiritus tartari, cui fluidum quoque obtentum (ponderis drachmarum XI.) in ceteris qualitatibus, e. g. sapore, colore aureo, simile erat; cum sale tartari, parum effervescens. Residuum sal erat alcalinum, carbonacei modo quid continens; sed alcalini volatilis ne vestigium quidem prodens. Sicco jam præteribo pede, singularem acidi fortis (exp. 53.) vi ignis mutationem in mite, quæ etiam in terra foliata tartari, sale acetosellæ et tartaro ipso obtinet, et a destructione quadam acidi, phlogisto intime mixti pendere videtur, nisi forsan hoc acidum (ut cl. PRIESTLEY celeberr. collega tuus, illustris HUNTERE, in clyssi nitri præparatione contendit) in aeris singularis speciem transeat.

Modus acidum pinguedinis fumans obtinendi mihi huc usque laboris et tædii plenus fuerat: peractis nimirum novem destillationibus (exp. 1–9.) et rectificatione (exp. 46.) acidum erat saturandum sale alcalino, quod evaporandum, calcinandum, iterum solvendum et inspissandum, priusquam vitrioli oleum, purum ex eo acidum expelleret (exp. 53.). Eundem nunc

finem obtinendi methodo in compendium redacta, mihi in votis erat; quod, non sine spe quadam sequenti modo conficere tentabam.

## E X P. LVII.

Imposui vesicæ et alembico, cupreis, stanno intus obductis, sebum depuratum, leni igne substrato nil nisi aquam emittens, quam autem, illo adaucto, sequebatur fluidum viridescens. At eodem tempore stannum variis in alembici locis, præcipue in tubo huic appposito, fundebatur et in externam superficiem penetrabat. Finita destillatione, in excipulo inveni acidum et oleum, citra expectationem meam, utrumque fluidum, nec ut antea coagulatum, quamvis residuum totum fere in carbonem versum esset. Hac quidem ratione laborum compendium quoddam repereram, a repetendis destillationibus et fusionibus absolutus; acidum vero non cupro solum inquinatum erat; sed vasa etiam ignis vi ita læsa erant, ut nonnisi magna adhibita opera, aliis laboribus inservire iterum possent.

Spreta itaque hac methodo, votis non ex omni parte respondente, alia occurrit, periculum videlicet instituendi cum solutione sebi in sale alcalino, seu cum sapone. Verisimile enim mihi videbatur, illud dum solveret adipem, acidum præcipue, in hoc contentum, esse arrepturum: quo facto, si oleum saponis posset separari a sale segneriano, tunc statim ad illud stadium processum pervenirem, quod haud sine mora, modo in exp. 46. eram assecutus: quæ autem separatio facillima mihi videbatur, quia sapo a quovis acido, nec non salibus mediis quibusdam, in partes dirimitur: quo destructo itaque, oleum filtro separare a fluido aquoso, hoc evaporare, tunc addere vitrioli acidum, mens mihi erat. Hæc ponderans, percepi saponem communem non posse adhiberi, quia tum lixivium ex cineribus paratum varia  
salia



salia media contineat, cum sal culinare adhibeatur ad saponem ex aqua separandum, quod pro parte huic se jungit. Saponis ita speciem mihi met ipsi confeci.

## E X P. LVIII.

Calcis vivæ recentis libram dimidiam itaque bene obrui cum salis tartari libra una, et linteo leviter tecta tam diu reposui, donec calx findi et dehiscere incipiebat. Tunc adfudi aquæ calidæ libras sex, quæ coctæ in vase ferreo ad quartæ partis consumptionem, per linteum densum transcolui (quod autem lixivium nunc ovum recens sustinebat). Quartam hujus partem, aqua tantisper dilutam coxi cum sebi libra una, donec maxima humiditatis parte evaporata, quam optime inter se coire inciperent. Affusa nunc lixivii reliqua parte coxi lenissimo igne, continuata agitatione, usque dum mixtura pellucida et quasi mucilaginosa adparebat, et frigefacta gelatinæ instar concreescebat, saponi communi, antequam sal culinare adjicitur perfecte similis. Separando nunc iterum oleo, a sale alcalino nihil mihi magis idoneum videbatur, alumine, quia partim minimi constat, partim non timendum esset, illud via humida, quod ab acidis mineralibus expectare mihi fas fuisset, salem segnerianum esse destructurum, quo facto acidum pinguedinis in auram abiisset; aluminis vero acidum tantum modo terræ suæ ipsius actionem infringentis deponit, quantum salis alcalini liberi invenit.

## E X P. LIX.

Gelatinæ itaque exp. anteced. in aqua solutæ injeci alumen pulverisatum, quod eodem momento vi quadam oleum coactum in superficiem urgebat. Hoc per cochlear cribratum sublato, iterum alumen adjeci; atque eodem modo perrexi usque dum post novam ejus additionem, nihil coacti superficiem

occuparet amplius\*. Fluidum colatum (ut terra aluminis, et particulæ quædam olei coacti separarentur) ad siccitatem evaporavi†.

## E X P. LX.

Ut acidum, quod exspectabam, ex sale segneriano expellerem, alumen, adhibere mihi occurrebat, ut eo certius acidum illud a vitriolico liberum obtinerem. Quamobrem addebam 2. partibus salis 1. partem aluminis usti, quas igni fortiori balnei arenæ exponebam. Peracta destillatione in excipulo inveni acidum fumans, ejusdem naturæ ut illud exp. 53. eoque modo finem concentrandi processus obtinuisse lætus perspexi. Attamen illi acido animadverti odorem quemdam sulphureum esse admixtum; et licet cl. BEAUMÉ (Chym. exp. t. 1. p. 335.) affirmet, parum acidi ex alumine expelli vi ignis solius; tamen a vero propius abesse in hoc processu mihi videbatur partes oleosas, massæ nostræ salinæ adhuc adhærentes, separationem acidi a terra aluminosa promovisse. Qua re motus, potius oleum vitrioli adhibere decrevi, quia tum ignis gradum poscat minorem, cum tota massa salina minor fiat.

## E X P. LXI.

Tribus partibus massæ nostræ salinæ ‡ affudi unam olei vi-

\* Reiteratis periculis hanc erui partium proportionem. Libris x. gelatinæ, in aqua solvendæ, addantur successive aluminis unc. xxii. (quarum aqua crystallisationis erit circa unc. xi. terra aluminosa oz. iv. cum dimidia). Mixtio hæc, colata, evaporata, dat salis unc. xxi. cum dimidia, quod ex tartaro vitriolato, sale segneriano et aluminis haud destructi parte compositum est.

† Si fluidum illud ad crystallisandum reponere placet, adhibito studio, tartarus vitriolatus, et superfluum alumen quoad maximam partem inde separari, et quod reliquum est, inspissari tunc potest: quo facto massa salina valde imminuitur.

‡ Optima proportio hæc est: salis nostri (exp. LIX. not. \*)  $\frac{1}{2}$  adduntur, olei vitrioli unc. iv. cum dimidia; acidum transtillatum adjicitur reliquæ massæ  $\frac{1}{2}$ , ut hoc modo rectificetur. Tunc habebis circiter unc. v. acidi limpidi fumantis.

trioli,

trioli, quod statim vapores griseos extricabat, acidum pinguedinis redolentes. Minor caloris gradus sufficiebat omni expellendo acido: nam et maximus nihil amplius educebat præter quasdam guttas olei ex bruno rubri\*.

Ut cognoscerem, an vitriolico acido nostrum sit inquinatum, partem ejus quandam superfudi soluto saturni saccharo, qua metallum exturbatum non iterum solvebatur adjecto vini aceto, quamvis illud digestionem et ipsa coctione sedimenti tentaverim†. Detecto itaque acido vitriolico, separandum illud esse ab acido nostro putavi, si nimirum, novæ massæ salinæ adjectum, iterum evocaretur: qua videlicet methodo acidum vitriolicum sali alcalino nubens, pinguedinis acidum expelleret‡.

\* Notatu dignum est, quod quavis via post evocatum acidum obtinerem aliquid salis ammoniaci animalis sicci, quod scilicet in aqua solutum sensum frigoris excitabat, cum calce viva tritum odorem spargebat alcali volatilis, et cum acido vitrioli, illum acidi pinguedinis. Quod vero sal alcalinum volatile, cum non inesse videbatur adipi, ut lateret in sale tartari (cf. cl. WIEGLEB. de salibus alcalinis) vel ut hoc, ope olei, volatile sit redditum (de quo autem dubito) necesse est.

† Cl. RETZIUS in Act. Acad. Stockh. t. 32. p. 216. contendit, certum hoc esse indicium absentis acidi vitrioli, si sedimentum ex saturni saccharo soluto per adjectum acidum ortum, resolveretur addito nitri acido; sed contrarium expertus sum. Adjectis enim drachmæ i. acidi nostri, guttis olei vitrioli quatuor, sedimentum quidem in saturni saccharo solutooriebatur; non statim solvendum per nitri spiritum, sed decantato a sedimento fluido, et adjecta nova nitrosi acido portione, hoc utique disparebat. Quod si vero eodem menstruo plumbum præcipitabatur, hoc per unc. iv. aceti vini nullo modo rursus dissolvere valebam, quamvis drachm. v—vi. sufficerent dissolvendo sedimento, absque guttis iv. acidi vitriolici exorto.

‡ Hac methodo in usum vocata, etiam uti licet cineribus clavellatis ad faciendum saponem nostrum; nam rectificando acidum nostrum super nova massæ salinæ quantitate, quodlibet acidum minerale illo pinguedinis admixtum, in illa massa remanet.

## E X P. LXII.

Acidi itaque nostri unc. iv. novæ massæ salinæ unc. i. adjectas leni igni exposui; quo facto in excipulum transtillabat acidum fumans, coloris limpidi, quod saturni saccharo soluto admixtum sedimentum quidem producebat, ad resolvendum adjecto vini aceto.

## E X P. LXIIf.

Concentrati acidi nostri vim in metalla experiundi nunc animus erat; illudque applicandi auro, quamvis spei haud multum rationes theoreticæ injicerent, fore ut solveretur. Auri itaque, ferri vitriolo præcipitati, granis iv. affudi acidi unc. i. quod leni calore digestum limpidum colorem in aureum mutaverat; magna licet metalli quantitate in fundo adhuc remanente; cujus vero coloris rationem peregrino cuidam, auro forte adhærenti, potius tribuens; idem cum hujus foliolis periclitatus sum; sed pari eventu. Quam itaque solutionem apparentem ut promoverem majore calore adhibito, acidum cum foliolis retortæ parvæ indidi aliamque implevi cum eodem acido atque granis iv. platinæ, et ex utraque fluidum, coquendo, evocavi, quod rursus residuis affudi et digessi. Color in utroque fluido aureus erat, metallorum majori quantitate licet in fundo remanente. Phænomeni haud expectati novitate perculsus, atque de alijs metallis solvendis cogitans, foliola argenti nostro acido immisi; quæ vero, cum discerperentur, fluidumque aureum colorem indueret, non potui, quin ex hoc colore, solutioni argenti minime convenienti in eam inciderem opinionem, illum a solo ipso acido pendere.

## E X P.

## E X P. LXIV — LXXIV.

Unam itaque acidi limpidi unciam coquendo destillavi ad dimidiam; quo facto residuum aureo conspicuum erat colore; finita vero destillatione, fluidum in excipulo transparens erat, in retortæ fundo vero animadvertēbam circulos brunos concentricos. Fluidum ex excipulo infundebam novæ retortæ puræ, iterumque destillabam ad siccitatem, remanente pari materiæ brunæ quantitate. Eandem operationem repeti oēties eodem modo; et ultima vice eandem, quam prima, repperi residui quantitatem; quod vero perfecte ficcatum, in aqua plane non et in ipso ejus acido, modo difficulter solvebatur; quin nitroso acido, nisi calore adjuto, haud cederet. Acidum nostrum fumandi vim amittebat; ejus vero acredo minime quavis destillatione sic decreſcebat, ut differentia sensibus percipi posset; quæ autem satis conspicua erat, si quod prima vice destillaverat, eum fluido quartæ destillationis, vel hæc cum oētava compareretur.

Notatu dignum utique est, quod hoc acidum distillando, vel digerendo colorem suum mutet, et quod antea totum volatile erat, nunc sedimentum dimittat, et acredinis vim perdat, ita ut a vero propius abesse videatur, quod pertinaciori adhibita opera tandem penitus destruat: qua itaque ratione medium esse nostrum acidum censendum est inter acida mineralia, acetumque, eaque acida, quæ, ut tartarus et acetosellæ sal, sine integra virium jactura plane transillari nequeunt.

Convictus hisce rationibus de fallaci auri in acido soluti augurio ex colore aureo; alia institui hac de re experimenta.

## E X P. LXXV.

Auri puri bracteolam, et platinæ granula quædam in vasis bene occlusis per 6. hebdomadas calori fornacis exposui: fluidumque tunc decantavi, ut adjecto sale tartari viderem, an pars quædam ex illo præcipitaretur\*, quod quidem minime eveniebat. Ex hac vero mixtione calori iterum exposita, pulvis descendebat, qui, fluido decantato, aqua edulcoratus et siccatus albi coloris erat†. Quæ quidem terræ species hoc modo non solum non effervescebat: sed etiam digestionem adhibita difficillime solvebatur; alia et contraria ratione intermissa nimirum siccatione se habebat; cui solutioni, an quid metallici insit, ut explorarem, tincturam sulphuris volatilem beguini (metallorum optimum proditorem) adjeci; sed præcipitatum sulphur ejusdem erat coloris, ut cum puro acido factum.

Hiscæ observationibus inductus pulverem illum terram meram esse opinor, quam acidum nostrum secum attollebat, quæque terrarum alcalinarum communium qualitatibus haud induta, ob volatilem naturam ad illam fluoris mineralis pertinere videtur; quod vero assertum experimentis probare, parca sedimenti quantitas vetuit.

\* Mirum forsitan posset videri, quare solutionem stanni in aqua regia non adjecissem: negare quoque non possum, me ab illa in periculo quodam adhibita, rubri coloris vestigium observasse: sed ab eadem statim abstinui, cum casu viderem, fluida supernatantia præcipitationibus (e solutionibus plumbi stanni, reguli antimonii, bismuthi et mercurii per acidum nostrum factis) confusa, invicem, novum præbere, sedimentum, rubello colore conspicuum, cujus causa, ut infra patebit, in stanno latere videtur.

† Idem fere phænomenon observavi, miscendo et digerendo solutionem alcalinam, cum pinguedinis acido, quod cum argento et bismutho antea digesseram.

E X P.

## E X P. LXXVI.

Auri nunc calcis per sal tartari paratæ gr. viii. cum acido nitroſi unc. dimidia per manus ſpatium digeſſeram, cujus tamen magna adhuc pars in fundo vaſis remanebat. Fluido colato addidi tincturam ſulphuris volatilem, quo facto mixtum colorem e coarctato griseum adeptum erat. Subſidentia facta, colatoque tunc fluido, reſiduum in filtro ſiccatum e nigro flavum erat, auri ſoluti præſentiam ſic demonſtrans; quod autem luculentius adhuc apparebat, evaporata parte quadam illius ſolutionis, e qua tunc cryſtalli e flavo brunæ, figuræ incertæ prodibant.

## E X P. LXXVII.

Difficultatem ſolvendi auri vincere cogitabam addendo alia acida. Pari itaque aureæ calcis portioni, affundebam acidi pinguedinis guttas 40. quibus in uno vaſe addebam guttas 20. acidi niſtroſi puri, in altero tantundem ſpiritus ſalis. In priori vaſe ſtatim fere conſpiciebam bullulas aereas ſeſe extricantes, atque ſolutionis initium indicantes: poſterius nullam mutationem patiebatur. Utrumque poſtea leni calore fovi; ſed licet ſolutio in priori increſceret; tamen in poſteriori nullum ejus apparebat veſtigium. Utriuſque fluidi guttas 8. infundebam in duas ſtanni ſoluti atque diluti portiones, quarum prior purpuram ſtatim dimittebat, poſterior, mutato haud colore turbidum, modo aliquatenus eyadebat.

## E X P. LXXVIII.

Quo ex periculo cum ſpem haurirem, fore ut aurum metallicum ipſum ſolverem, ejus bracteolæ ſuperfudi guttas 80. acidi pinguedinis, et guttas 20. acidi niſtroſi puri. Eodem fere momento tota ejus ſuperficies bullulis aereis tecta, placidaque erat



illius solutio; additis vero adhuc acidi nitrosi guttis 20.; hæcce magis, magisque increſcebat, donec, calore adjuta, totam bracteolam consumeret. Quod quidem phænomenon argumentum eſſe poteſt discriminis, acidum noſtrum inter et illud ſalis, intercedentis: certo enim certius eſſe videtur, duas partes acidi ſalis fumantis, et unam aquæfortis, aurum non poſſe diſſolvere; imprimis ſi digeſtio non adhibeatur: quam ob rem itaque pinguedinis acidum ſuo jure inter efficaciora acida locum ſibi vindicare videtur.

## E X P. LXXIX.

Platinæ calcem ex aqua regis per tartari ſal præcipitatam, eodem modo (exp. 76.) tractavi, cujus ſolutio colata cum beguini tinctura ſedimentum deponebat obſcurioris coloris, quod in filtro collectum, ſiccatum ex flavo brunum erat. Solutionis altera pars evaporata in cryſtallos oblongas ex flavo brunas concreſcebat; quarum copia illa, ex auro obtentas multum ſuperabat.

## E X P. LXXX.

Argenti foliola ab acido quidem noſtro corroſa, parum tamen ſolvebantur; paucae interim ejus particulæ cupro immiſſo adhærebant; et ſalis acidum affuſum, aliqualem ſed vix conſpicuam præcipitationem producebat. Argenti vero calx continuata digeſtione ſolvebatur, ex quo, adjecta tinctura beguini, metallum ſulphuri adhærens fundum petebat, quod in filtro collectum, et ſiccatum nigreſcebat. Solutio evaporata in cryſtallos coibat, albo colore (ut argentum nitratum) haud conſpicuas; id quod acido longa digeſtione obſcuriorem colorem induenti tribuo, oleum vitrioli (minime vero ſalis ſpiritus) ſolutioni admixtum, ſedimenti quid procreabat.

## E X P.

## E X P. LXXXI.

In mercurium nostrum quidem acidum haud agere videbatur; sed hoc ab illo altera vice abstrahendo, observavi, metallum in paucō fluido residuo, mobilitatem suam solitumque splendorem perdidisse, et in massam quasi cylindrā, circumagendo vitrum, coire. Quæ quidem omnia fluido transtillato iterum affuso adhuc persistabant; admoto vero digestionis calore rursus evanescabant. Postquam omne acidum ad siccitatem abstractum erat, superficiē retortæ majorem partem velut amalgamate obductam deprehendi, quod non a mercurii globulis, sed a solidis quasi argenteis pendere videbatur, quæ parti abstractæ iterum affusæ pro tempore innatabant, tandem vero subsidentes, solvebantur; id quod plus una vice observavi; colata solutio cupream laminam dealbabat; sed illa ab adjecto sale communi non turbabatur; mixtum vero hoc fluidum, colatum, cupro adhuc argenteum induebat colorem.

## E X P. LXXXII.

Facilius adhuc calx (a mercurio sublimato cum tartari sale remixto, exorta) ab acido nostro suscipiebatur absque caloris adjumento. Quod vero mixtum (exp. 81. motus) destillare decrevi in arenæ balneo, cujus sub initium fluidi aliquid transibat, postea vero paululum adaucto calore, pars quædam sublimata alba collo retortæ adhærebat. Hæc autem nova mercurii sublimati species, aquæ deinde immissa, difficilius, adhibita etiam digestionē, solvebatur, et adjecto sale tartari sedimentum album deponebat. Cum beguini tinctura commixta statim in substantiam nigram, paulo post in cinnabarim mutabatur; cupro attrita, exacte licet sicca, albam ei inducebat superficiem; (id quod mercurius quoque sublimatus communis præstat). Nos-

trum itaque acidum, excepto salis acido, solum omnium est quod sublimatum siccum cum mercurio facit, atque (quod singulare utique est) leñiori adhuc caloris gradu, surgit. Arena enim inferius circumdata retorta incumbbat, receptaculo ex tenui ferri lamina confecto, atque lateribus coctis superposito: quo modo ignis vis et ob deficientem craticulam, et ob parvum foci spatium magna haud esse poterat.

## E X P. LXXXIII.

Cuprum abique prævia digestionē solvebatur, teste viridi fluidi colore; quæ vero adhibita, illud promovebat. Evaporando quidem crysalli apparebant, in aere autem mox deliquescentes.

## E X P. LXXXIV.

Ferri facilis solutio saporis erat adstringentis; in crysallō coibat aciculares, humiditatem atmosphæricam vix attractantes.

## E X P. LXXXV.

Plumbum difficiliter solvitur, et potius modo corroditur, minium vero acidum facile subit, quod jam, ante plenariam solutionem, rubrum colorem exuit; albo tunc pulveri simile. In solutionis (quæ a sale culinari haud mutatur) saturatæ superficie oriuntur crysalli ad 2''' fere longæ, pugiliaculi forma præditæ, tandem desidentes; quarum sapor dulcis quid habet.

## E X P. LXXXVI.

Regulus antimonii, abstrahendo acidum ab illo, solvitur: si vero fluidum in excipulo illi in retorta adhuc residuo, affunditur, lacteum assumit colorem, nec pelluciditatem nisi adhibita digestionē

digestione recuperat. Evaporata solutio in crystallos abit, in aere haud deliquescentes.

*E X P. LXXXVII*

Solutio zinci facili negotio peragitur, quæ singulari sapore metallico prædita est, et adjecto sale tartari sedimentum album deponit, quod flammæ admotum (ut zinci flores) flavum evadit.

*E X P. LXXXVIII*

Stanni Malaccensis rasura ab acido nostro in pulverem flavescentem corrodebatur; et majori quidem adhuc vehementia, si calori exponeretur, sic ut hujus unæ dimidia destruendis illius scrup. ii. sufficeret. Odor ex mixtura surgens maxime ingratus, et illi fere similis erat, quem salis acidum cum zincæ edit. Fluidum paucum supernatans turbidum erat, quod omni studio decantatum chartæ bibulæ superfudi; sed haud mutatum transibat: quin chartam istam duplicatam, immo quadruplicatam penetraret; haud minus, quam antea turbulentum. Aliquo tempore post pulvis subsidebat flavescens, atque ei supernatans, fluidum pellucidum colore pulchre roseo splendebat; quod decantare frustra tentabam; simulac enim vitrum solum modo tangebam, summa imis miscebantur; et fluidum chartam bibulam, eadem sub specie turbida penetrabat; postea iterum subsidens. Cui vero colori rubro sedimentum rubellum (exp. 75. n. \*) tribuendum videretur.

Hanc corrosam calcem stannicam digerebam aqua destillata, quæ colata dein atque evaporata relinquebat sal album facile deliquescens: quod si vero eidem calci affunderem novam aciditatem, ut eandem ex toto in fluidum roseum iliquerem, illa quidem mox colore isto ornata, sed sedimenti quantitas haud imminuta erat; quæ nunc calori exposita, illud non solum non solvebat, sed et gratum colorem cum flavo commutabat.

## E X P. LXXXIX.

Bismuthum, per longum quidem temporis spatium cum acido licet digestum, non solvebatur; contrarium vero eveniebat adhibita calce (quam ex solutione illius cum nitroso acido multa aqua diluto facta) dejecerat admixtum sal alcalinum. Quæ vero solutio ab aqua adjecta lactescebat; et sedimentum album dimittebat; ab acidis vero vitrioli et falis mutationem nullam experiebatur.

## E X P. XC.

Cobalti ex smalta reductus regulus, eodem ut bismuthum modo digestus, eadem pertinacia acido resistebat: calx vero, ex nitrato cobalto per tartari sal deficiens, eidem facile cedebat\*. Cujus solutionis drach. tribus cum unam nitri adjicerem, et destillationem instituerem, hac ad medium perducta vapores percepi flavos, versus illius finem rubros (exp. 117). Partem falis jam crystallifati et viridis distincte observabam dealbari a vaporibus nitrosis; quod deinde solutum in aqua, speciem constituebat atramenti sic dicti sympathetici, ex flavo viridescentis.

## E X P. XCI.

In regulum niccoli (repetita sæpius, etiam adjecto carbonum pulvere, tostione, eique interposita fusione cum nitro, calce, et borace paratam) nulla fere erat acidi nostri etiam cum illo digesti actio. Adjectum ei sal tartari nihil deturbabat, quum e con-

\* Hæcce solutio, calori exposita, partem aliquam dimittit, quæ postea non iterum suscipitur; id quod in aliis solutionibus e. g. niccoli et bismuthi, quoque observavi.

trario tinctura beguini, parvam metalli quantitatem sulphuri unitam dejiciebat. Calx e niccolo nitrato per alcalinum salern præcipitata absque digestionem dissolvebatur ab acido nostro, cujus color evadebat viridescens, et adjectis acidis vitrioli et nitri nihil dimittebat.

## E X P. XCII.

Arsenicum album magna cum difficultate, digestionem licet adhibita, solvebatur sic, ut ejus scrupulus unus ab acidi uncia dimidia vix susciperebatur. Caloris ope autem hanc subibat major illius copia, quam eo absente sustentari poterat; quo factum ut tunc parvæ crystalli fundum peterent. Si nostræ solutioni immittebatur cuprum, illud nihil dejiciebat, sed potius pro parte ab acido suscipiebatur. Parte aquosa sensim evaporante, sal apparebat e viridi cœruleum; sub finem vero aliud saturate viride; luculento argumento, prius esse compositum ex arsenici acido et cupro; alterum ex nostro acore, eodemque metallo.

Altera pars solutionis adjecto sale alcalino nihil dimittebat; ea quoque pars, cujus fluidum in auras abiērat, adjecto oleo tartari per deliquium ex integro solvebatur; quod vero non ita multo post sedimentum deponebat, nova salis tartari portione non auferendum; idque sal neutrum arsenicale fuisse, parca aquæ quantitate haud solvendum censeo.

## E X P. XCIII.

Magnesium mineram Ilfeldensem digerebam cum nostro acido, quod illam in initio corrodebat, pulveremque nigrum a crystalliformi minera separabat; deinceps vero illam in parca haud quantitate solvebat. Quod cum aliis metallis digestum acidum colorem brunum induerat, cum magnesio nullam patiebatur mutationem; et odorem spargebat ad illum solutionis stannæ

acce-

accedentem; saporis erat metallici. Addita aqua illam turbidam paululum reddebatur. Vitriolicum acidum vero nihil dejiciebat (indicio, terram calcaream mineræ haud inesse) cum sale fixo alcalino mixtum; sedimentum copiosum dimittebat, quod statim novo adjecto acido iterum disperebat; si vero conversa via sal alcalinum sedimento majori adhuc copia superfunditur, illud dissolvit; adjecto vero tunc acido, e. g. salis, metallicam partem iterum deponit. Tinctura beguini acida nostræ solutioni admixta colorem præ se ferebat rubellum, et quæ copiosissime dimittebat, ficcata, ejusdem adhuc erant coloris.

Absolutis nunc eis, quæ de actione acidi nostri in metallis dicenda erant, superest adhuc, ut videamus, quam rationem habeat idem acidum metallicis solutionibus adjectum.

*Præcipitationes metallorum, in alijs acidis solutorum, per acidi pinguedinis admixtionem exortæ.*

E X P. XCIV.

*Aurum.* Hujus metalli solutionem in aqua regis (quam vitro, in quo medicamenta fluida asservari solent, infuderam) acri libero exposueram, ex qua hac ratione tandem pulchræ crysalli flavæ exortæ erant, quæ figuræ salis communis appropinquantes, ex superimpositis lamellis angulatis constabant, in aere vero per plures hebdomadas detentæ haud diffluebant\*. Quas quidem crysallos in aqua solvebam destillata simplici, quæ, adjecto nostro acido, sedimentum flavum dimittebant. Decantato fluido, illud lavabam nova aquæ quantitate, quæ defusa, cum alia ejusdem destillatæ portione adjecto, sedimen-

\* Aliquo tempore post inveniebam hæc crysallos motum velut intestinum assas, volumine adjecto in farinosam vel flocculentam substantiam mutatas.

tum per plures dies digessi, colavi, evaporavi, atque hoc modo obtinui residuum, aquam ex aere attrahens.

E X P. XCV.

**Platina.** Ex ejus solutione in aqua regis acidum nostrum deiciebat pulverem fere aurantium, qui, edulcoratus, multa aqua superfusus, digestus,colato atque evaporato fluido, residuum exhibebat e griseo flavidum, quod in aere auro minus difflebat.

E X P. XCVI.

**Argentum.** Quod acidum pinguedinis ex argento nitrato precipitabat\*, coloris erat grisei paululum in rubellum vergentis; et, edulcoratione pregressa cum aqua digerebatur. Cujus parti uni addebam guttas aliquot acidi vitriolici, atque imperfectaoriebatur precipitatio: altera, evaporata, relinquebat residuum aquam valde attrahens: argentum vitriolatum statim adjecto acido sedimentum album dimittebat; luna vero cornua cum acido digesta haud mutata videbatur.

E X P. XCVII.

**Mercurius.** Acidum nostrum album producebat sedimentum ex mercurio nitrato†. Sed (quod notatu quam maxime dignum mihi videtur) idem ex sublimato aliquid exturbabat‡: affuso enim illo, mixtio paulo post lactea evadebat; pulverem

\* Promptior adhuc erat precipitatio per sal ammoniacum crystallum.  
† His quoque magis adhuc precipitabatur adjecto sale ammoniaco animali.  
‡ Mercurius, acido salis optime mixtus, addito acido vitriolico minime mutatur; atque si aqua, sale selenitico forte, sublimatum solventes, aliquidi flavi deponant, (cf. cl. BEAUME Chym. t. II. p. 434.) quod et ipse vidi, tribuendum mihi hoc videtur affinitati duplici, quum nimirum acidum salis terram calcaream quoque amet, eamque ob rationem metallum vitriolico acido coale colligens.



dein album deponens; quod eo citius evenit, si mixtum digeritur. Fallor; aut hoc sedimentum album ex sublimato corrosivo hac ratione ortum criterij instar esse potest ad distinguendum acidum nostrum ab alijs, præcipue vero a muriatico. Illud vero sedimentum ablutum, digestum, in aqua solvebatur, eique immixtum cuprum albescebat; eadem quoque solutio evaporata residuum dabat album, in aere non liquefscens.

## E X P. XCVIII.

*Plumbum.* Quæ e saturno nitrato descendebant crysalli parvæ aciculæ formam gerentes, edulcoratæ, aqua digesta facillius solvebantur; acidoque tunc vitriolico admixto sedimentum dimittebant. Dissipata illius solutionis humiditate superstes erat pulvis aquam parum attrahens.

## E X P. XCV.

*Bismuthum.* Nitrosum acidum, metallo ope digestionis solvendo destinatum, tanta aquæ copïa dilutum erat, ut, solutione peracta, nova illi admixta aqua nihil præcipitaret. Simulac vero acidi nostri guttæ aliquot accedebant, pulverem album dejiciebant, qui ablutus, cum aqua digestus, solutione colata, evaporata, residuum album progignebat facillime liquefscens.

## E X P. C.

*Regulus antimonii.* Saturata ejus in aqua regis solutio, addita aqua destillata, turbida evadebat; quam colatam, nova adjecta aqua non amplius mutabat: ab affuso vero nostro acido, statim oriebatur sedimentum album, ex quo aqua extrahebat partem fluido dissipato conspicuam, quæ, humiditatem iterum attrahens, in crysallum parvas tenues coibat.

**E X P. CI.**

*Stannum.* In aqua regis solutum metallum deiciebatur ab acido nostro, et colorem induebat ex flavo brunum. Præcipitatum ablutum, et cum aqua digestum sal progignebat albidum facillime liquecens \*.

**E X P. CII.**

*Cuprum.* Hoc neque ex vitrioli costuleo, nec ex cupro nitrato per pinguedinem præcipitabatur.

**E X P. CIII.**

*Ferrum,* nec nitratum, nec vitriolatum, cum acido nostro mixta sedimentum dimittebant.

**E X P. CIV.**

*Zincum,* nec nitratum, nec vitriolatum mutabantur ab acido nostro adjecto.

**E X P. CV.**

*Cobalti regulus,* nitratus ab acido pinguedinis affuso nullam perturbationem passus est.

**E X P. CVI.**

*E niccoli regulus* nec nitrato, nec salito acidum nostrum aliquid extricare valebat.

**E X P. CVII.**

*Arsenicum* nitratum, acido nostro admixtum, nullum sedimentum deponebat.

\* Hæc quæ ab acido nostro natales ducunt præcipitata, nil esse videntur quam salia metallica in aqua soluta difficilia.

## E X P. CVIII.

*Magnesium* nitratum nullam conversionem expertum est a nostri acidi admixtione.

*Acidorum diversorum actio in sal segnerianum\*.*

Quod *nitricum* acidum nostrum, acorem ex sale hoc medio expellat; jam supra protuli.

## E X P. CIX.

*Nitricum acidum*. Dubius salis nostri drachmis affudo tantundem aquæ fortis, ut vocant, duplicis (quam peracta ejus præcipitatione destillaveram) nulla effervescentia sensibili inde oriente. Quod peracta destillatione excipulo inerat fluidum, saporis acidi nostri proprii erat, sed odori æminatum aliquid aquæ fortis. Decompositum vero fuisse sal nostrum, ejusque acidum expulsum, demonstrat præcipitatio celerrima e saturno nitrato, per fluidum illud, quod destillando obtinueram.

\* De figura hujus salis monenda quædam adhuc mihi supersunt. Quod ad terram foliatam accedat (Exp. nov. p. 2.) asserui, Segnerum secutus, sed cum illud majori in copia paravissem, et ~~namque~~ salinam perscrutarem, inveni illam crusta superius lectam, qua vero ablata, huic adharebant multæ crystalli, tres ut plurimum lineas longæ, pugionis quadrangularis forma conspicuæ, quarum duo latera opposita ceteris angustiora erant. Si salis alcalini quantitas minima hæc adjecta est, et crystalli ficcantur super charta bibula; illæ in aere haud dissolvuntur: qua ratione, ut et crystallorum forma, a terra foliata tartari, mirum, quantum differunt. Segnerus vero in hanc observationem ex eo ~~quod~~ videretur, quod salis medii parcam modo quantitatem pararet, quapropter ~~lætentes~~ sub crassa saline crystallos, ob spatii, et materiæ defectum, observare haud posset. Forſan nec acidum ejus ab oleo particulis satis liberum; nec ad saturationem aliud sal alcalinum, quam cineres clavellati, adhibitum erat.

E X P. CX.

*Muriaticum acidum.* Aequale nostri salis medii et acidi muriatici pondus commiscebam: quæ destillata, exhibebant acidi pinguedinis drachm. ii. proprio odore præditas, et e sublimato corrosivo pulverem album præcipitantes.

E X P. CXI.

*Aceti vini optimi* drachm. vi. superfudi salis nostri drachm. ii. quod ex hisce destillando obtinui fluidum odoris erat acetii, et sublimato corrosivo admixtum, illud intactum relinquebat. Cum in maiorem rei explanationem residuo in retorta adlicerem salis spiritum, et destillarem, acidum, adipis se predebat jam odore, et præcipitatione mercurii sublimati.

E X P. CXII.

*Fluoris acidum* pari pondere nostro salii admixtum celerrime in illud penetrabat, ut siccum videretur. Quod non nisi magno caloris gradu adhibito prodibat fluidum, fluoris acidum haud mutatum erat: quod etiam saturno nitrato affusum ejus pelluciditatem (ut ei moris est \*) non tollebat; hac ratione quam maxime abhorrens ab acido pinguedinis.

E X P. CXIII.

*Phosphori sal.* Huius in aqua soluti unc. dimidiam addebam salis medii nostri drachm. ii. Sub initium destillationis fluidi quid transibat, quod vero nil nisi aqua erat. Qua ex excipulo evacuata, ignem adaugebam + quo adhuc aliquid ex massa extorquebam, quod vero nec acidum erat, nec saturni saccharum decomponabat.

\* Cf. cl. SCHEELE in Comment. Stockh. vol. XXXIII.

+ Idem erat huius gradus, qui ad sublimandum sal ammoniacum animale requiritur, et maiorem adhuc adhibere dubitabam, cum sal nostrum solo igne vehementiori jam decomponatur.

E X P.

## E X P. CXIV.

*Arsenici albi* et salis nostri paullisper flavidi parem quantitatem in pulverem album comminuebam, quorum actionem in se reciprocam ut promoverem, adjiciebam aquæ destillatæ drachm. II. quæ omnia leni calore digerebam. Elapsa vix horæ una quarta, pars quædam pulveris nigrescebat, et parieti in forma annuli nigri \* fortiter adhærebat, reliqua massa salina ab illo separata erat. Destillando (eodem, ut in exp. præcedenti, ignis gradu) parum fluidi obtinui, quod nec lapidum erat, nec plumbum ex ejus saccharo præcipitabat. In collo retortæ parum sublimati, ejusque tenuis reperi.

## E X P. CXV.

*Cobaltum nitratum.* Salis nostri drachm. unam immisi solutionis cobalti nitratæ unc. dimidiæ, et fluidum penitus evocaui. Sal exsiccatum in retorta viridi gaudebat colore, quod refrigeratum hunc commutabat cum albo: illud solvebam in aqua destillata, quæ nunc exhibebat novam atramenti sympathetici speciem, cobaltino communi haud absimilem, in luteum modo magis vergentem colorem.

## E X P. CXVI.

*Salis ammoniaci animalis* (ex acido pinguedinis et alcali volatili compositi) drachm. II. commiscebam cum lapidis sic dicti hæmatitæ granis xv. quæ igni exposita sublimatum exhibebant; quod vero, peracta operatione immutatum esse sal ammoniacum, reperiēbam, relicto in fundo hæmatite. Eadem iterum mis-

\* Nil hoc mihi videtur, nisi subita reductio quædam arsenici albi in regulum, destillatione quoque peracta nigrum adhuc erat et durum; quod abrasum, desegebat massam albam, nonnihil firmam. A vero proprius mihi esse videtur, reductionem hanc ortam esse philogisto, sali flavido in hærente.

cebam,

cebam, addendo aquæ aliquid in meliorem utriusque combina-  
tionem: sed tamen eadem eveniebant omnia.

*Actio acidi pinguedinis in salia media.*

E X P. CXVII.

*Nitrum.* Hujus exacte depurati drachmis II. superfudi acidi nostri tantundem, quod illud cum aliqua vehementia solvebat. Retortâ vix arenæ calidæ immissa, flavida jam a vaporibus evadere mihi videbatur: qui color semper saturatior fiebat, donec eundem ruborem acquireret, qui adhibito vitriolico acido conspicitur. Fluidum in excipulo odore acido, nitroso communi, præditum erat, cui etiam aliquid acidi pinguedinis admixtum; nam argentum purum non solito ab aqua forti more solvebatur; potius crusta tegebatur satis crassa, colore dâute hepatico conspicua.

E X P. CXVIII.

*Sal-muriaticum.* Hujus drachm. II. solvebam in acidi nostri pari pondere. Destillatione ad finem tendente, distincte observabam vapores griseos: odor fluidi in excipulo contenti, erat acidi muriatici: sed ut hoc cum certitudine quadam constaret, et an acidum pinguedinis admixtum sit, exploraretur, res haud parum perplexa erat, cum utrumque magnam inter se alat similitudinem. Cui vero fini respondere stannum posse judicabam; eamque ob causam miscebam, 1. aquæ fortis guttas 80 cum guttis 40 spiritus salis; 2. eandem aquæ fortis et spiritus salis copiam (ut 1.) cum acidi pinguedinis guttis 40; 3. aquæ fortis guttas 80 cum acidi pinguedinis guttis 40. Unicuique harum mixtionum destinabam stanni malaccensis scrup. II. et in quodvis vitrum, a quavis stanni portione, tenuissima fila non prius

prius immittebam, quam olim injecta, absque caloris auxilio, plane disparuissent. Quævis enim mixtionem in metallum agebant: N. 1. maxime, N. 3. minus, N. 2. minime: cum vero N. 1. solvendo ulterius stanno impar esset, grana hujus 7. adhuc supererant: solutio erat pellucida, et absque ullo sedimento. N. 2. maxime turbidum erat, coloris e griseo flavidi; sedimento copioso nigrescente refertum; stanni residui pondus erat gran. xvii. N. 3. exhibebat solutionem pellucidam, cum parvo in brunum vergente sedimento; stanni granis 9 adhuc superstitibus. Hisce periculis peractis, quæ regulæ instar esse debere mihi animus erat, admiscebam guttis 80 fluidi ex destillatione obtenti guttas 160 ejusdem aquæ fortis. In qua mixtione dissolvebam peditentim (eisdem phaenomenis apparentibus, ut in N. 1.) stanni fila, donec drachma 1. consumpta esset; sed sedimenti nigri quid in fundo hærebat. Quibus ponderatis, concludendum mihi esse videtur, quod fluidum in excipulo repertum acidum fuerit muriaticum; id quod præprimis ex vaporibus griseis, et ex magna stanni copia, quæ absque magna sedimenti copia solvebatur, colligo, pinguedinis vero acidum haud admixtum fuisse; ea ex ratione censeo, quia solutio clara, nec sedimentum brunum erat; nigrum vero illud ex conortum fuisse judico, quod salis spiritum (concentrationem, quam opinabar) non diluerem nitrosi acidi quantitate sufficiente.

## E X P. CXIX.

*Terra tartari foliata.* Huic addebam æquale pondus acidi nostri, cum qua paululum effervescebat; peractaque tunc destillatione acetum excipulo inesse prodebat odor, nec non deficiens in sublimatum corrosivum actio.

## E X P. CXX.

*Sal mirabile Glauberi.* Quamvis haud expectari posse videbatur acidum nostrum expulsum esse vitriolicum, tamen experientiam consului. Utroque æquali pondere mixto et destillato, inveniēbam in excipulo fluidum, quod præter odorem acidi nostri, sulphureum quoque admixtum habebat. Quamobrem illud affudi solutioni plumbi in acido nostro factæ, quæ sedimenti albi quid dimittebat; indicio, parvam acidi vitriolici quantitatem divulsam esse a sale alcalino; quod phlogisto acido adhuc adhærenti tribuo, quo nimirum vitriolici acidi pars volatiliior reddita videtur.

## E X P. CXXI.

*Tartarus tartarizatus* in aqua solutus, adjecto acido sedimenti magni copiam dimittebat, quod fluido decantato, veri cremoris qualitates demonstrabat.

Liceat hisce experimentis quædam addere de similitudine et cognatione acidi nostri et muriatici. Utrumque cum alcali volatili constituit sal ammoniacum siccum, et cum magnesia alba, sal valde diffuens; utrumque argentum et mercurium e menstruis præcipitat; ab utroque soluto regulo antimonii, adjecta aqua turbatur, et metallicam partem deponit. Eandem cognationem hoc quoque indicare videtur, quod acidum muriaticum solutionis argenti et mercurii in acido nostro non præcipitet. Sed magna etiam inter utrumque intercedit differentia: nostri nimirum acidi intima combinatio cum oleosis partibus; sal calcareum haud diffuens; naphthæ facilis genesis; argenti et mercurii solutio via simplici humida; et hujus præcipitatio ex sublimato corrosivo.



His inter se comparatis, characteres cognationis utriusque acidi, quam illi discriminis, potiores mihi omnino esse videntur.

Hæc sunt, illustris et celeberrime HUNTERE, pericula, quæ cum acido pinguedinis hucusque institui, omnia. Minime me fugit, materiam hanc nondum esse exhaustam; multaque adhuc esse supplenda, quæ intimiori nostri acidi cognitioni favent; imprimis explorandos esse affinitatis gradus, quos metalla contrahunt cum acido nostro. Quibus vero omnibus exantlandis minime deero, si modo cognovero, labores meos huic rei impenso plane haud displicuisse eruditis, imprimis illustrissimæ Regiæ Societati, cujus aliquem in me favorem quam maxime cupio. Interim vale, vir celeberrime; meque tibi ut habeas commendatissimum, majorem in modum oro rogoque. Dabam Helmstadii Idibus Decembr. 1780.

III. *Observations on the Bills of Mortality at York.* By William White, M. D. F. A. S.; communicated by Nathaniel Pigott, Esq. F. R. S.

Read December 6, 1781.

**F**AITHFUL and accurate registers of the number of births and deaths kept in different places are of great importance to the community. The statesman, the philosopher, and the physician, are equally interested in inquiries which infallibly shew us the real state of the nation, as to population, healthfulness, and, as connected with the latter, virtue and temperance.

It must give great pleasure to a reflecting mind, to find, from undeniable proofs, that this nation appears to be, in the above respects, in a general and progressive state of improvement. The births have become more numerous, the deaths fewer, in proportion in almost every place where the registers have been consulted: for proof of this I refer to the Transactions of the Royal Society, vol. LVII. LIX. LXI. LXIV. LXV. &c. and to a publication of Mr. WALES, F. R. S. intituled, An Inquiry into the present State of Population in England and Wales, lately published.

It would not perhaps be difficult, and as a physician I could with pleasure attempt the investigation, to discover the various

causes to which such effects may be attributed; but here a wide field offers itself to our examination. It will, however, be necessary just to point out such as affect this city in particular, in a subsequent part of this paper.

Mr. DRAKE, F. R. S. in his *Antiquities of York*, has given us the number of births and burials for 7 years, from August 5, 1728 to August 5, 1735, inclusive. This gave a favourable opportunity of comparing our present state after an elapse of 45 years. In order to this, the different parish registers were carefully examined from January 1, 1770, to December 31, 1776, inclusive: I added the number of males and females for the latter term, which Mr. DRAKE omitted.

TABLE

TABLE I. The number of births and burials in York from August 5, 1728, to August 5, 1735.

The different parishes.	Births.	Burials.
All Saints, Pavement,	123	218
All Saints, North-street,	101	111
St. Crux,	132	159
St. Cuthbert's,	55	80
St. Dyonis,	92	106
St. Helen's,	113	122
St. John's,	136	173
St. Laurence,	60	77
Martin's, Conyngs-street,	73	110
Michael le Belfray,	310	327
St. Mary's, Castle-gate,	150	221
St. Michael, Spurrier-gate,	198	216
St. Martin's, Mickle-gate,	92	117
Bishophill the elder,	103	117
Bishophill the younger,	57	73
St. Maurice,	55	158
St. Margaret's	118	147
St. Olave's,	147	181
St. Saviour's,	70	103
St. Sampson's,	188	228
Christ Church,	140	119
Trinity, Goodramgate,	143	144
Trinity, Mickle-gate,	129	152
Dissenters,	18	29
	<hr/> 2803	<hr/> 3488

The burials, therefore, exceeded the births 685 in 7 years, or 98 annually.

TABLE

TABLE II. The number of births and burials from January 1, 1770, to December 31, 1776, inclusive.

The different parishes.	Births.	Burials.
All Saints, Pavement, -	240	153
All Saints, North-street, -	96	88
St. Crux, - - -	146	109
St. Cuthbert's, - -	102	126
St. Dyonis, - - -	109	96
St. Helen's, - - -	96	76
St. John's - - -	183	124
St. Laurence, - - -	97	83
Martin's, Conyng-street, -	104	74
Michael le Belfray, - -	297	298
St. Mary's, Castle-gate, -	159	210
St. Michael's, Spurrier-gate. -	151	113
Martin's, Mickle-gate, -	82	98
Bishophill the elder, -	124	151
Bishophill the younger, -	121	92
St. Maurice, - - -	76	138
St. Margaret's, - - -	182	142
St. Olave's, - - -	234	296
St. Saviour's, - - -	96	108
Sampson's, - - -	174	184
Christ Church, - - -	147	110
Trinity, Goodram-gate, -	161	118
Trinity, Mickle-gate, -	122	164
Dissenters, - - -	24	24
	<hr/> 3323	<hr/> 3175

Decreased in burials 313, or  $44\frac{2}{7}$  annually.

Births increased 520, or  $74\frac{2}{7}$  ditto.

Births exceed the burials 148, or  $21\frac{1}{7}$ , ditto.

TABLE

TABLE III. The number of births and burials, with the proportion of males and females, annually, from January 1, 1770, to December 31, 1776.

	Births.	Males.	Females.	Burials.	Males.	Females.
1770	467	237	230	417	203	214
1771	451	225	226	485	225	260
1772	490	238	252	508	220	288
1773	474	244	232	499	241	258
1774	453	214	239	382	173	209
1775	490	255	243	488	237	251
1776	498	255	243	396	177	219
	<u>3323</u>	<u>1666</u>	<u>1657</u>	<u>3175</u>	<u>1476</u>	<u>1699</u>

Number of males born in 7 years 1666, or 238 annually.

Number of males buried in 7 years 1476, or 210 $\frac{2}{7}$  annually.

Number of females born in 7 years 1657, or 236 $\frac{4}{7}$  annually.

Number of females buried in 7 years 1699, or 242 $\frac{5}{7}$  annually.

TABLE IV. Mortality of the seasons.

Winter.	Spring.	Summer.	Autumn.
Jan. 320	Apr. 277	July 220	Oct. 237
Feb. 282	May 265	Aug. 237	Nov. 230
Mar. 316	June 274	Sept. 225	Dec. 292
<u>918</u>	<u>816</u>	<u>682</u>	<u>759</u>

In order to find the number of inhabitants in any place, where, either from its bulk, or other reasons, a numerical survey cannot be obtained, two methods may be made use of. The first is, multiplying the number of houses by the medium of inhabitants in each. The second is, one recommended by Monf. MOHEAN, in a work, intituled, *Recherches et Considerations*

*tions sur la Population de la France.* He found, by very laborious calculations, that the number of inhabitants may be known by the births, the latter being to the former as nearly 1 to 27.

By an account given into the House of Commons in March 1781, the number of houses in York subject to the new house-tax was 2285: if to those be added such as were too small to come under the tax, which may probably amount to one-third more, the total of the houses in York will be about 3000. This number multiplied by  $4\frac{1}{2}$ , which is nearly the medium of people in a house, gives 12,750 for the number of inhabitants.

By the second rule we have 12,798 for the number of inhabitants, which is the result of 474, the average annual births, multiplied by 27.

The remarkable coincidence of the above methods of calculation makes it very probable, that if we estimate the number of inhabitants at 12,800, we shall not be far from the truth.

However this may be as to the exact number of inhabitants, it affects not the principal end of the present inquiry, which is to shew how we are improved in population and healthfulness within 40 years past.

In order to prove this, we must find the number of inhabitants in the year 1735, from tab. 1. We there find the average annual births to be 400; this multiplied by 27 gives 10,800 for the number at that time. This number divided by the average annual deaths 498, gives the proportion of deaths 1 in  $21\frac{1}{4}$ . Such was the state of this city as to mortality 46 years ago.

Very different from this is our present situation, the proportion of deaths being now decreased to 1 in  $28\frac{1}{4}$ , which is the quotient of 12,800, the number of inhabitants divided by 453, the

the present average of annual deaths. This is certainly a great rise in the scale of healthiness. From being near as fatal as London we have become less so than many country places, as will appear from the following comparative view of the proportion of deaths in different places.

At Vienna,	-	1 in 19½ dies every year
London,	-	1 in 20½
Edinburgh,		1 in 20½
Berlin,	-	1 in 21
Rome,	-	1 in 22
Amsterdam,		1 in 22
Dublin,	-	1 in 22
Leeds,	-	1 in 22
Northampton,		1 in 26
Shrewsbury,		1 in 26
Liverpool,		1 in 27½
Manchester,		1 in 28
York,	-	1 in 28½

Hence in 1735, at York it would require 21½ years to bury a number equal to that of its inhabitants; but in 1776, 28½ years would be required for the same. One third less die yearly now than in the former period; and we are certainly advancing still higher, for in 1777 the births were more than in any former year, being 516, the burials 464.

As there is no settled manufactory here, there is little increase or decrease of the people by acquisition or emigration, and probably what may happen in either case is nearly balanced by the other.

It appears from tab. 4. that the summer season is by much the healthiest at York; autumn the next; then the spring; winter being by far the most fatal. Dr. PERCIVAL found much



the same to be the case at Manchester. At Chester Dr. HAYGARTH says November was the most sickly month. The differences in the registers make it impossible to give the diseases of which the individuals died; yet a general idea of this may be obtained from the same table. By the care and attention of the present archbishop of this province, this may be easily perfected in future periods.

It appears from hence, that our diseases are chiefly of the inflammatory kind, which physicians know to be the general attendants of the winter and spring months. The disorders of the summer and autumn are more particularly such as arise from putrescency and acrimony, such as slow and remitting fevers, dysenteries, cholera's, and the like, those then being with us the healthiest seasons shew that we are not subject to putrid diseases. Dr. WINTRINGHAM has given us an account of the weather and the corresponding diseases at York for sixteen years successively, in his *Commentarium Nosologicum*, to which learned work I refer the curious reader for further satisfaction upon this subject.

Among the general causes of our increasing population and healthiness we may enumerate the introduction of inoculation, which has been the means of saving a number of lives; improvements in the treatment and cure of several disorders, the cool regimen in fevers, the admission of fresh air, the general use of antiseptic medicines and diet, have doubtless had a salutary and extensive influence upon the health of mankind, and have much obviated the malignity of some of our most dangerous diseases. To these may be added a general improvement and greater attention to nature in the management of infants.

After

After the general causes of healthiness, such as are particular, or of a more local nature, come under consideration. In this respect the city of York has been much improved within a few years past. The streets have been widened in many places, by taking down a number of old houses built in such a manner as almost to meet in the upper stories, by which the sun and air were almost excluded in the streets and inferior apartments. They have also been new paved, additional drains made, and, by the present method of conducting the rain from the houses, are become much drier and cleaner than formerly. The erection of the locks, about four miles below the city, has been a great advantage to it: for, before this, the river was frequently very low, leaving quantities of sludge and dirt in the very heart of the city, also the filth of the common sewers which it was unable to wash away. The lock has effectually prevented this for the future, by the river being kept always high, broad, and spacious; and has thus contributed to the salubrity as well as beauty of York. In the above improvements, in others that are intended to take place, in the care and expence necessary to keep in proper repair the public walks about the city, the magistrates have exerted much public spirit, and have added to the health as well as consulted the convenience of its inhabitants.

York,

Sept. 8, 1781.



IV. *Account of a monstrous Birth. In a Letter from John Torlefe, Esq. Chief of Anjingo, to the Hon. William Hornbey, Governor of Bombay; communicated by Dr. Lind, F. R. S.*

Read January 7, 1782.

H O N. S I R,

Anjingo,  
April 5, 1780.

**A**S I know you are curious with respect to the productions of nature, I have taken the liberty to inclose you a drawing of a child which a Nair woman was delivered of the 28th of March at midnight, and which lived till the 1st of April in the morning. In the afternoon I went to see it in company with Mr. HUTCHENSON and Dr. CROZIER. You will see by the sketch that it had but one body, at the extremity whereof were two heads, one larger than the other. It had four hands and arms perfect, two legs on one side ~~its~~ body, and one on the other, which began on the middle of its back, and appeared by nature intended for two by its size and from the appearance of the foot, which looked as if two had been squeezed or rather mashed together. It had but one navel and one anus, but two genitals of the female. It was fed during  
its





its short existence by hand with goat's milk. It is remarkable, that one head would sleep whilst the other was awake; or one would cry, and the other not. They both died at the same instant. Almost all this town went to see it, the like having never been heard of before. The mother is a stout woman; and I saw four of her children at her house, the youngest of which was six years old, all healthy and perfect.

I am, &c.

*V. Experiments with Chinese Hemp Seed. In a Letter from  
Keane Fitzgerald, Esq. to Sir. Joseph Banks, Bart. F. R. S.*

Read January 17, 1782.

S I R,

Poland Street,  
Dec. 17, 1782.

EVERY thing extraordinary in art or nature falls, in some measure, within the views of the Royal Society; but how far the following account of what appeared to me an extraordinary production may be worthy of being communicated to that learned body, is submitted entirely to your consideration.

A few grains of Chinese hemp-seed had been given to me by the late Mr. ELLIOT, brother to General ELLIOT, who had formerly resided for some time in China. He told me, the hemp in that country was deemed superior to that of any other, both for fineness and strength, and wished I would try whether it would come to maturity in this kingdom. He gave me between thirty and forty grains of seed for the purpose, which I laid by, as I thought, carefully, with intent of sowing them the spring following, which is the usual time of sowing hemp in this country; but I had unluckily forgotten where I laid them, and did not find them till the beginning of last June, by which time I imagined them to be very unfit for vegetation; but as I concluded they would be still more so by keeping them till the succeeding April, I had them sowed the 4th day of that month, and was much surprised to find that thirty-two  
of

of the seeds had vegetated strongly, and grown to an amazing size, several of the plants measuring in height more than fourteen feet, and seven inches nearly in circumference, by the middle of October following, at which time they came into bloom. There were from thirty to forty lateral branches on a plant; these were set off in pairs, one on each side of the stem pointing horizontally; the others at about five or six inches distance from them, pointing in different directions, and so on to the top, the bottom branches of some measuring more than five feet, the others decreasing gradually in length towards the top, so as to form a beautiful cone when in flower, which were unluckily nipped by a few nights frost that happened to be pretty sharp towards the end of the month; and the plants began to droop at the beginning of November at which time I had them pulled up by the roots.

As I was but little acquainted either with the cultivation of the seed, or preparing the plants afterwards for the production of hemp, and as these plants were very different in their size from any I had ever seen, the best method that occurred to me was, that of steeping them in water, where I let them remain for a fortnight, and then placed them in an upright position against a south wall to dry and bleach.

On trying whether the hemp could be easily separated from the woody part, I was agreeably surprised to find, that on peeling a few inches longitudinally from the root, the whole rind, from the bottom to the top, not only of the stem but also of all the lateral branches, stripped off cleanly, without breaking any one of them. The toughness of the hemp seemed to be extraordinary, and upon drying and beating divides into an infinity of tough fibres. The plants when stripped are quite white, and when the lateral branches are cut off, appear



like handsome young poles. They are perforated in the middle, but the perforation is not larger than that of a goose quill, in a stem of more than two inches diameter. The woody part seems pretty substantial, and if they should be found of any duration, might be applied to many useful purposes; or if not, I should imagine they would produce plenty of good ashes by burning.

The rough hemp that has been peeled from the thirty-two plants, when thoroughly dried, weighed three pounds and a quarter; but I do not think it had come to full maturity, though I can hardly doubt but the plants would have come to perfection if the seed had been sown in the proper season. The summer was remarkably dry, notwithstanding which, although the situation they were placed in was very warm, and the ground not rich, I found, on measuring the plants at different times, that they had grown nearly eleven inches *per* week.

As the culture of so valuable a kind of hemp as this promises to produce appears to be of consequence to a maritime and commercial kingdom, I have applied to the Directors of the East India Company, to give proper orders to their factors and super-cargoes in China, to procure some of the best seed that can be obtained; and send, even a small parcel, by each of their returning ships, which they have very obligingly promised; and from what has already appeared, there can be no doubt of its continuing in a state fit for vegetation for a much longer time than is usually required for that voyage.

If the seed should arrive in safety, I can hardly doubt of obtaining the assistance of the Society established for the Encouragement of Arts, Manufactures, and Commerce; and should expect, from their wonted assiduity and liberal disposition of proper

proper rewards for the culture and manufacture of so valuable a commodity, to see it as successfully carried to perfection as several other branches have happily attained by their care and protection; and shall think myself very happy in being any ways instrumental in forwarding so good a purpose.

As you did me the favour of examining the plants in a growing state, I need not trouble you with any specimens from them; but if they should be deemed worthy of being laid before the Society, I shall send some of the hemp in the state it was peeled, and a piece of the stem it was peeled from, as also specimens of the leaf and flower, for their examination.

I am, &c.

VI. *An Account of some Scoria from Iron Works, which resemble the vitrified Filaments described by Sir William Hamilton. In a Letter from Samuel More, Esq. to Sir Joseph Banks, Bart. P. R. S.*

Read January 17, 1781.

S I R,

**I**N the very accurate account given of the eruption of Mount Vesuvius in the month of August, 1779, in a letter from Sir WILLIAM HAMILTON, printed in the Philosophical Transactions, vol. LXX. part I. p. 42. et seq. among many other equally curious informations, it is said, “ Long Filaments of “ vitrified matter, like spun-glass, were mixed with and fell with the ashes.” And in a note annexed it is also said, that “ during an eruption of the volcano in the Isle of Bourbon in “ 1766, some miles of country, at the distance of six leagues “ from the volcano, were covered with a flexible capillary yellow glass, some of which were two or three feet long, with “ small vitreous globules at a little distance one from the other.”

There appeared to me on reading these passages an exact similarity between these productions of the two volcanos and some scoria I had received from a worthy friend, who is master of one of the largest works in England for smelting iron. In a letter accompanying the specimen, he writes, “ I have sent “ a specimen of some slag, or vitrified cinder, which has by

“ the reverberation of the blast from the Tweer \*, been drawn  
“ out whilst fluid into long cobweb-like threads (sometimes  
“ ten or twelve feet in length) and affixed itself to the beams,  
“ &c. of the bellows room.”

Whoever has attentively viewed the large furnaces wherein iron ore is smelted by coak, will readily allow, that they present the most striking resemblance (however diminished) of that most tremendous of all appearances, the eruption of a volcano; and that the most exact pictures hitherto seen of the flowing of the lava from the one is shewn by the running of the slag from the other: this has induced me to lay before you, for the inspection of the Royal Society if you judge it worthy their attention, some of the scoria in its capillary state, and with all due deference to the acknowledged abilities of Sir WILLIAM HAMILTON, to submit to your consideration, and that of the learned Body over which you so deservedly preside, whether the fine filaments may not be produced in the eruption of the great furnaces of nature, by means similar to those by which we see them formed in the furnaces of art. Sir WILLIAM seems to think, “ That (what he calls) the natural spun glass which  
“ fell at Ottaiano, as well as that which fell in the Isle of  
“ Bourbon in 1766, must have been formed, most probably,  
“ by the operation of such a sort of lava as has been just described (that is, perfectly vitrified) cracking, and separating  
“ in the air at the time of its emission from the volcanos, and  
“ by that means spinning out the pure vitrified matter from  
“ its pores or cells, the wind at the same time carrying off those  
“ filaments of glass as fast as they were produced.” See p. 81.

\* The Tweer is that opening through which the air is driven by the bellows into the body of the furnace.

That some of the fine filaments found after the eruptions of the volcanos were formed in this manner is not unlikely: but as we see about the iron furnaces the vitrified scoria drawn into fine threads, of very considerable length, by the simple action of the wind from the bellows, is it not very probable, that the far greater part at least of those filaments scattered over the land, and which were found two or three feet long, were drawn out before the ejection of the lava from the crater by the force of those violent torrents of wind which must be required to support and actuate so intense a fire as at those times fills the body of the mountain?

In all matters of this kind there is great scope for conjecture, and much must be allowed to it; and I have presumed to submit this opinion to you, not with an intention to dispute the probability of what has been already advanced on this head, but to point out from what occurs immediately under the eye of every workman about our iron furnaces, some easy and simple mode of accounting for so singular a phenomenon, and as an introduction to my presenting to the Royal Society a specimen of so curious a production.

The extreme fineness to which these filaments are reduced, and their brittleness, render it almost impossible to convey them to any distance, preserving at the same time any considerable length of the fibres; these which I have now the honour to lay before you resemble cotton in appearance, but if examined with a microscope will be found in all respects similar to those described by Sir WILLIAM HAMILTON.

I am, &c.



VII. *An Extract of the Register of the Parish of Holy Cross, Salop, being a Third Decade of Years from Michaelmas, 1770. to Michaelmas 1780, carefully digested in the following table. By the Rev. Mr. William Gorfuch, Vicar; communicated by Dr. Price, F. R. S.*

Read January 17, 1781.

		1771	1772	1773	1774	1775	1776	1777	1778	1779	1780	
Baptized	{ Males	23	20	19	20	18	31	16	18	20	18	203
	{ Females	16	18	16	12	23	14	17	27	22	17	182
Buried	{ Males	16	19	12	11	13	28	13	12	23	13	160
	{ Females	13	20	17	10	6	21	14	15	13	22	151
												385
												311
												74

Increase 74

	1771	1772	1773	1774	1775	1776	1777	1778	1779	1780	Total
	M	F	M	F	M	F	M	F	M	F	M
Under a month	0	1	—	1	1	2	1	0	2	—	1
From mo. to 1 yr.	2	3	2	2	0	3	0	1	2	2	4
1 to 2	1	0	1	3	2	0	1	4	—	—	2
2 to 5	1	0	4	2	1	—	1	0	5	7	3
5 to 10	—	2	2	0	1	—	0	1	3	1	0
10 to 15	—	—	—	—	—	—	—	—	—	—	—
15 to 20	—	1	2	—	—	—	—	—	—	—	—
20 to 25	1	0	—	—	—	0	1	1	0	—	1
25 to 30	—	1	0	—	—	3	0	1	1	1	0
30 to 35	—	—	1	2	1	0	—	0	1	0	1
35 to 40	2	0	—	—	1	2	0	1	1	—	1
40 to 45	2	2	0	1	1	0	1	0	—	0	1
45 to 50	2	0	1	1	0	2	—	—	0	1	0
50 to 55	1	0	1	1	1	1	2	1	0	1	2
55 to 60	2	1	—	—	—	1	0	—	1	0	1
60 to 65	0	1	0	1	0	1	0	3	0	1	2
65 to 70	—	1	0	—	—	—	—	—	2	2	—
70 to 75	0	2	1	3	4	0	1	1	2	2	0
75 to 80	2	1	1	—	—	1	0	2	0	1	—
80 to 85	0	2	1	1	0	2	—	1	2	4	1
85 to 90	—	1	0	—	—	—	—	0	1	—	—
90 to 95	—	—	0	1	0	1	0	1	—	—	0

An:

An actual survey was made in 1775, when the number of the inhabitants was found to be total 1057: of which under ten 287, and above seventy 37, viz. from 70 to 75, males 12 females 10 = 22. From 75 to 80, males 8 females 11 = 19. From 80 to 85, males 8 females 6 = 14. From 85 to 90, males 1 females 1 = 2.

An actual survey was made in the year 1780, when the number of inhabitants were 1113.

There remains alive in 1780.

Under ten years of age,	males 155	} 293
	females 138	

From 70 to 75	males 6	} 17
	females 11	

From 75 to 80	males 5	} 13
	females 8	

From 80 to 85	males 2	} 6
	females 4	

From 85 to 89	males 2	} 3
	females 1	

Distempers

**Distempers and Casualties from 1770 to 1780.**

Accidents	-	6	Meazles	-	-	7
Apoplexy	-	5	Palfy	-	-	9
Astma	-	5	Pleuresy	-	-	1
Cancer	-	2	Rheumatism	-	-	1
Chin-cough	-	5	Small-pox	-	-	43
Consumption	-	62	Sore-throat	-	-	8
Child-bed	-	3	Stone	-	-	2
Convulsions	-	23	Suddenly	-	-	2
Dropfy	-	20	Teeth	-	-	2
Drowned	-	3	Untimely	-	-	4
Fever	-	15	Worms	-	-	3
Jaundice	-	2	The remainder died of a natural decay, without any ostensible Distemper.			
Lues venerea	-	1				
Mortification local	-	5				
Mortification intestine	-	10				

The number of inhabitants actually surveyed every five years for thirty years.

In 1755	-	1049
1760	-	1048
1765	-	1096
1770	-	1046
1775	-	1057
1780	-	1113

The increase of 48 persons in the year 1765 was owing to the ingress of four numerous families into large houses, which were almost uninhabited for many years before.

The



The decrease of 50 persons in the year 1770 was occasioned by the demolishing of nine houses, in order to open a way to the new stone bridge built over the river Severn.

On Good Friday, 1774, there happened a dreadful fire which originated from a chimney, and extended on both sides of the street to the distance of half a mile: the wind blowing with great violence, the flames in a few hours consumed 48 houses, being generally thatched buildings. In this conflagration 179 of the inhabitants lost their dwellings, but immediately provided themselves with lodgings within the parish, and of the number of sufferers only 24 persons went out of the parish, and returned no more. The ground vacated by the houses burnt is now, in 1780, built upon, and mostly covered with little tenements fitted for poor inhabitants to live in, and made so commodious as to receive a greater number of inhabitants than they did before the fire in 1774; so that the families, whose number in 1770 were 240, are in the year 1780 increased to 246 nearly, and perhaps will be increasing when more buildings shall be erected.

Houses pay window lights 65. The new house tax paid by 36 houses. The first decade was published in the Phil. Transf. vol LII. part I. art. 25. The second decade was published in the Phil. Transf. vol. LXI. art. 6. p. 57. See also Dr. PRICE's Observations on Reversionary Payments, ed. 1771. p. 192. and note *a.* also p. 259. and 263.

The taking account of the marriages in this parish cannot be of any use in political arithmetic, because it is the custom of the fixed inhabitants to go out of the parish, and be married in distant churches; and the weddings performed in this church are generally between strangers who occasionally reside here so long as to make a place of abode according to the act  
of

of parliament made in 1754. Dr. HEBERDEN hath made a very proper use of the number and proportion of marriages in the island of Madeira; but then it was an island, and all are confined to their own constant residence. If the whole island of Great Britain was universally to be included in the account of the number of marriages, it would be very useful and compleat.

Many young people have gone out of this parish to supply the navy and army, but probably the same number would have emigrated, to be apprenticed and follow different occupations.

The public register is too general for the purposes of political arithmetic. The extracts here made are drawn from private papers, kept with great care and exactness, so that the births and burials of the fixed inhabitants are not rendered incorrect by the accidental ingress of foreigners, or temporary egress of the real inhabitants.

VIII. *An Experiment proposed for determining, by the Aberration of the fixed Stars, whether the Rays of Light, in pervading different Media, change their Velocity according to the Law which results from Sir Isaac Newton's Ideas concerning the Cause of Refraction; and for ascertaining their Velocity in every Medium whose refractive Density is known. By Patrick Wilson, A. M. Assistant to Alexander Wilson, M. D. Professor of Practical Astronomy in the University of Glasgow; communicated by the Rev. Nevil Maskelyne, D. D. F. R. S. Astronomer Royal.*

Read January 24., 1782.

UPON the supposition that the refraction of light is caused by a certain action of gross and sensible bodies upon it, Sir ISAAC NEWTON has demonstrated, that the sines of incidence and refraction, when the rays pass out of one medium into another of different density, must always be in a constant ratio. This constancy of the ratio of the sines is agreeable to an universal experience, and has been called the law of refraction. Upon the same grounds he has also demonstrated, that the velocity of the rays must be greater in the more refracting medium in the inverse ratio of the sines. Of this property of refraction, however, we have hitherto had no evidence in the way of experiment. The ideas entertained by Sir ISAAC NEWTON, from which this property has been deduced, though they confess their great author, by a most beautiful simplicity, and

and by a very striking agreement with fact, have yet been deemed by some persons as not perfectly authentic. His contemporary LEIBNITZ and others have attempted demonstrations of the law of refraction from principles very different, and which do not lead to the opinion of the acceleration of light in the more refracting medium. At present it is proposed to point out a method of determining experimentally the law of the variation of the velocity of light, according to the change of the medium. If observations shall shew this law to be agreeable to Sir ISAAC NEWTON's conclusions, we shall then have a very strong additional evidence in favour of his principles. If, contrary to the most probable issue of the experiment, some unsuspected law should be discovered, we must, according to the rules of induction laid down by that great master in philosophy, so far restrict our general conclusions, and accommodate our ideas to the real condition of things.

The method of experiment at present alluded to is, that of observing the aberration of the fixed stars with a telescope filled with a dense fluid, such as water, or any other equally limpid and of greater refraction, fitted to bring the rays to a focus by the surface of the medium opposed to the object having a proper degree of convexity. It is enough at this time to suggest a general notion of the instrument, and we now proceed to explain in what manner it can assist us in the present inquiry.

Since aberration, taken in its enlarged sense, depends on the relative velocities of light and of the telescope, if the rays were really to move much faster or much slower in an unusual telescope of this kind, it seems to follow, that the quantity of aberration given in these circumstances, compared with Dr. BRADLEY's angle, would certainly indicate the new rate of velocity. Such an inference would certainly be just, and it is

upon these grounds that we propose to inquire into the velocity of the rays, as they move forward in dense media so applied to telescopes. Granting, however, for the sake of argument, that light moves down through such an unusual telescope with an increased velocity suited to the refractive density of the medium, it will by no means happen, that the aberration will be changed on that account. This proposition, which at first view may appear paradoxical, and even contradictory to what has been affirmed above, is however not the less certain, and may serve to shew, what caution is sometimes requisite in applying general principles to particular cases: for it shall be proved, that the aberration in such a telescope will precisely agree with that of Dr. BRADLEY's only in the case of the rays moving swifter in the watery medium than in air, in the ratio assigned by Sir ISAAC NEWTON, and that this sameness of aberration will itself be a proof of light being so accelerated within the telescope.

In the illustrations which follow, the reader is supposed not to be wholly unaccustomed to the distinctions betwixt absolute and relative motion, as this will prevent repetitions and all unnecessary prolixity.

Let ABC (fig. 1.) be the spherical refracting surface of such a telescope as has been described, and let the telescope be supposed to be at rest, or the velocity of light to be infinite with respect to that of the earth, and let GBMF be a line drawn from a star at G, in the pole of the ecliptic, through the center M of the refracting surface; the image of the star will be formed somewhere, as at F, in the line BF; and here the intersection of the cross wires made use of in observing must be placed. It is evident, that the star will be seen in its true direction FG; and we must conclude that to be its true direction, because we know

know that the ray GBF passes into the medium without being refracted by it, and BMF would be considered as the axis of the telescope.

Now let the spherical refracting surface with its wires, or the unusual telescope be carried laterally with the motion of the earth towards Q. Conceive GBF to be a line not partaking of this lateral motion, which at any particular moment passes thro' M, the center of convexity. Along this line suppose one of many rays to pass from a star situated in the pole of the ecliptic. Then will all the contemporary light of this pencil of parallel rays be made to converge so as to meet in a focus somewhere in the unrefracted ray BF. Let F therefore be the point in absolute space where the image of the star is so formed. Let the parallel motion of the telescope, whose refracting spherical surface is ABC, be in the direction of HF, and take FD to FB as the lateral velocity of the telescope to the velocity of light in air, and join BD: then it is manifest, that BD will be the position of a telescope such as Dr. BRADLEY's, when the image of the star is formed in the axis BD, and that IBG, or its equal FBD, will be the angle of greatest aberration.

Moreover, the velocity of the rays as they proceed to the focus F, after refraction at the surface ABC, being supposed the same as in air, it is evident, that the line DML drawn through D, and through the center of convexity M, must give the position of the axis of this kind of telescope, when the image of the star is formed there: for, by hypothesis, the image is formed in F in absolute space, and since BF is supposed to be to FD, as the velocity of light within the medium to the lateral velocity of the telescope, the point D of the axis DL will arrive at F, when the rays arrive there to form

the image. And the observer not knowing, or at present not taking account of, the lateral motion of the telescope, will suppose, that the line LMD joining the image of the star and the center of convexity M is the true direction of the star; just as before he concluded, that FMBG would be the direction of the star when the lateral motion of the telescope was supposed to be nothing. Hence it is evident, that the intersection of the cross wires, made use of in observing, must now be placed at D; or else, if those be still used that were before supposed to be at F, the refracting surface ABC with the line or axis BF must revolve about the center M till the vertex B comes to L and the cross wires F to D.

In like manner, if the velocity of the rays were increased after refraction at the spherical surface in any ratio, as that of DF to EF, the refraction continuing the same, then EMO drawn through the center of convexity would now give the position of the axis of the telescope necessary for receiving the image formed at F. For the space described by the rays in passing downwards to the focus, in this case and the former being equal, the times of their converging at F will be reciprocally as the velocities, or as EF to DF. But, on account of the equable lateral motion of the telescope, DF and EF will be as the times of the points D and E arriving at F: therefore, in the last case, the intersection of the cross wires supposed at E will meet the image at F, and accordingly the star will be seen in the axis.

Fig. 2. From what has been said it will appear, that if DF be taken to EF, as the sine of incidence to the sine of refraction peculiar to the medium which fills the telescope; then, from the property of the focus, we shall have this proportion, viz.  $BF : FM :: DF : EF$ . Hence the line EMO passing through

through M must be parallel to DB; but DB, as before, denotes the position of Dr. BRADLEY's telescope, when the aberration of the star is at its maximum, and EMO parallel to it, denotes the position of the water telescope, at the same time, upon the supposition that the velocity of the rays without and within be as EF to DF, or inversely, as the sines of incidence and refraction peculiar to water. Here then we discover what must be the law of variation as to the velocity of the rays, provided that the aberration given by such a telescope shall come out the same with that found by Dr. BRADLEY. It is the very same which follows from the Newtonian principles: for from the manner of observing, the angle of aberration is always determined by the position of the telescope necessary for having the image formed somewhere in the axis.

But supposing that in the course of observing with such a telescope, the aberration should come out different from what has already been ascertained by Dr. BRADLEY, it may next be enquired, how from the difference given the velocity of light within the telescope is to be deduced.

Fig. 3. Imagine then such a telescope actually to give FMD as the greatest angle of aberration, and let this be supposed greater than that of Dr. BRADLEY's, which, for example, let be FME. From what has been already said, the velocity of light corresponding to this last mentioned angle, is deducible from the known refraction of the medium which fills the telescope; and, by construction, the velocity corresponding to FMD, the angle given, must be to the former inversely as the tangents of these angles. From this consideration we have the following analogy for finding the velocity corresponding to whatever difference there may be observed between the two aberrations at present alluded to. The rule in all cases must be;



“ as the tangent of the observed angle is to the tangent of the  
 “ Bradleyan angle, so is the velocity of light deducible from  
 “ the hypothesis of the observed angle being the same with  
 “ that of Dr. BRADLEY to the velocity sought.” It has already  
 been shewn, how the former of these velocities can be universally  
 ascertained, from the known refraction of the medium which  
 is taken to fill the telescope, and therefore the last term of the  
 above proportion, which is the velocity sought, is thereby  
 given.

Fig. 2. In a telescope of this kind it will not have escaped  
 notice, that the ray BF, which, on account of its passing to  
 the focus unrefracted, may be called the axis of the pencil,  
 can never be found in the axis of the telescope EO, except at  
 the focus F, where D and F meet. That ray, however, OP,  
 parallel to BG, which falls obliquely on the axis of the tele-  
 scope EO, will continue to pass along it after refraction, and  
 for that reason it may be called the relative axis of the pencil.

This will appear, by considering that the particle of light,  
 which at any moment is refracted at the vertex O of the spheri-  
 cal surface, is found by hypothesis in the axis a second time,  
 when it meets the cotemporary light at the focus. But since  
 both the motion of the axis and of the particle is uniform and  
 rectilinear, the former cannot be found in the latter at two  
 different times, without being found in it continually during  
 the whole interval. In like manner, a part of every other ray  
 from the star, which successively falls upon the vertex, must  
 move relatively along the axis after refraction: and thus a con-  
 stant succession of these particles constitute a visual refracted  
 ray, whose relative path must always be in the axis OE.

All that has been shewn concerning the telescope already  
 considered, will receive still further illustration, by tracing the  
 motion

motion of this particular refracted ray till it arrives at the focus. This way of viewing the subject will also render the reasoning more general, and make it apply to telescopes when the dense fluid within is supposed to be confined by object-glasses of any figure. But in order to this, it will be convenient to premise, and briefly to demonstrate, what shall afterwards be referred to by the name of

**P R O P. A.**

Fig. 4. If any very small body or particle of light as it moves uniformly in the absolute path SB, has passed relatively along a part of the line CD, which advances equably and parallel to itself in the direction DK; and if at any instant the absolute path of the particle be changed into any other, as BR; I say, it will still pass relatively along the moving line, provided its velocity now be to its former velocity as the sine of the angle DBF to the sine of the angle DBR; these being the angles which the moving line BD makes with BF and BR the absolute path or direction of the particle in the two cases.

The construction of this figure is so simple, that it is unnecessary formally to point it out. Since, by hypothesis, the velocity of the particle along BR is to its former along BF as the sine FZ to the sine RT; or, on account of similar triangles, as DF to IR, and, on account of parallels, as DF to DW, it follows, that the time of its describing BR now, is to the time of formerly describing its equal BF, as DW to DF. But the line BD advancing with a uniform motion, the time of its arriving at W is to the time of its arriving at F, also as DW to DF. Therefore, when the particle arrives at R, the point D of the moving line will have arrived at W, and WRP will be its position. Hence the particle at that moment must be found in the intersection R of this line, with its absolute path BR. In the

same manner it may be shewn, that at any other time the particle will be found in the intersection: it, therefore, from the time of its direction being changed at B, must pass relatively along the moving line as before. By a small alteration in the construction, it may be shewn, that if the absolute path had been so changed at B as to have augmented the angle FBD, still the particle would have moved relatively along DB, provided its velocity after had been to its velocity before as the sine of FBD the first angle to the sine of the increased angle.

To apply, therefore, this proposition to the present investigation, let DB be conceived as the axis of a telescope perpendicular to the spherical surface of a refracting medium which accompanies it in its lateral motion, SB the absolute path of a particle of light which had passed relatively along DB produced, till its arrival at B, and BR its absolute path within the medium of the telescope. Then it is evident, that FBD, or its equal CBS, will be universally the angle of incidence, and RBD the angle of refraction. Hence, by prop. A. that ray of the parallel pencil which is refracted at O, the vertex of the spherical surface in fig. 2. must still pass relatively along the axis, provided the velocity within the telescope be to its former in air, as the sine of incidence to the sine of refraction. But the image of the star being produced by the meeting of all the contemporary light, will consequently be found in the axis, which, by hypothesis, deviates from the true place of the star by the same quantity as Dr. BRADLEY's angle; so that in this way of considering the matter, the same thing results which was formerly shewn in regard to a telescope so constructed.

By prop. A. it is also manifest, that whatever number of refractions that ray which falls upon the extremity of the axis suffers in pervading object-glasses of any figure, or even dense media

media beyond the object-glass if bounded by transparent planes to which the axis produced is perpendicular, yet if the velocities and refractions so correspond, still the ray in question will pass relatively along the axis till it meet the rest at the focus: for here the refracted ray in the first medium becomes the incident ray in relation to its path in the second, and this in its turn becomes an incident ray in relation to its path in the third medium, &c. and therefore by the prop. A. can never deviate from the moving axis whatever be the refractive density of the media, or however these are disposed in the order of succession. And since, by Sir ISAAC NEWTON's theorem, the ratio of the sine of incidence to the sine of refraction in the passage of a ray out of one medium into another, is compounded of the ratio which the former has to the latter in the passage of that ray out of the first medium into any third, and of the ratio of the former to the latter in the passage of the same ray out of the third medium into the second, &c. it follows, that if the velocities be related to the degree of refraction as before mentioned, the ray in the last dense medium will, notwithstanding any number of previous refractions by glasses, &c. have the same final velocity that would have been acquired on its passing immediately out of air into that medium. This being the case, it appears, that though the intervention of an object-glass may shorten the focal distance of such a telescope, yet it will not displace the image nor alter the rule of inferring the final velocity of the rays in the dense medium from the aberration given; at least when this is supposed to be the same with Dr. BRADLEY's.

Fig. 3. But further, if the aberration of such a telescope should differ from the Bradleyan one, and give, for example, the angle OMB, still the ray PO, which falls on O the vertex, must be considered as an incident ray, which, after refraction,

K 2

passes

passes along the axis. By prop. A. therefore, the velocity of the ray, whatever this may be after refraction, must be to that velocity by which it would have moved relatively in the axis, so inclined to its path, previous to the refraction, inversely as the sines of incidence and refraction. Now this being duly considered, it will be found that the velocity within the medium, corresponding to this supposed aberration, or the absolute velocity within the medium, must be to the velocity within the medium corresponding to the Bradleyan aberration, inversely as the tangents of these two angles: for let  $V$  and  $v$  express the velocities before and after refraction corresponding to the Bradleyan angle, and  $X$  and  $x$  the velocities before and after corresponding to the supposed uncommon angle,  $x$  being the actual velocity after refraction; then, because by prop. A. the antecedent is to the consequent, in both cases, in the same ratio, *viz.* as the sine of refraction to the sine of incidence, it will be  $V : v :: X : x$ , and therefore  $V : X :: v : x$ . But from the nature of the aberration  $V$  must be to  $X$  (this supposititious velocity before incidence) inversely as the tangents of the angles of the two aberrations. This therefore must be the ratio of  $v$  to  $x$ . But  $v$  is given as before shewn; therefore  $x$  the velocity within the medium corresponding to the supposed observed aberration is also given, and by the same rule as was found formerly in the case of the first telescope.

What has been at present advanced is unconnected with any hypothetical notions concerning the rays or the cause of refraction. Light has been considered only as something which moves uniformly from one place to another, and which is always refracted according to a known law. The first of these properties has been put beyond all doubt by the observations of Dr. BRADLEY and Mr. MOLYNEUX; and it is has been long known that the last is quite agreeable to experience.

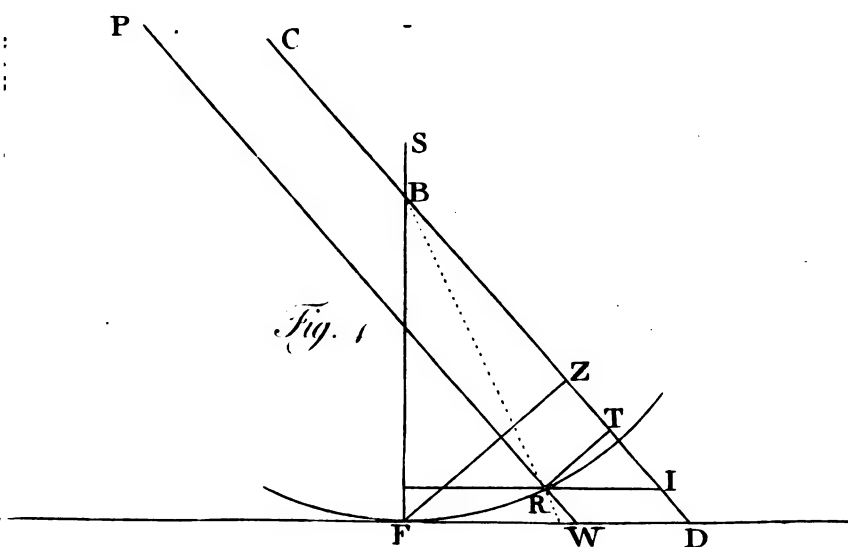
It has indeed always been taken for granted, that the velocity of the ray which passes through the center of convexity, represents the common velocity of all the contemporary light of the converging pencil. This may perhaps be reckoned a circumstance of which we have no proof. But it must be considered, that if the rays of light, after being variously bent towards the focus, were no longer to move with the same common velocity, the image formed at the focus of Dr. BRADLEY's telescope, would be elongated in the direction of the aberration. Those who have attended to this subject will be at no loss in discerning the reason of this. The extent of that lengthened image would depend upon the difference of velocity which would obtain among the converging rays, and would probably increase according to the largeness of the aperture of the object-glass. But such a phenomenon being contrary to experience, it follows, that the unequal bending of the rays does not give them unequal velocities, whilst moving in the same medium. This is another property with regard to the motion of light which may be considered as proved experimentally by Dr. BRADLEY's observations, and which doubtless would have occurred to him if he had had occasion to trace the refraction of a pencil of parallel rays at the object-glass of his telescope.

To conclude: in bringing this question concerning the velocity of light to the issue of an experiment, that fluid would doubtless be most proper for the telescope which absorbs the fewest rays, and possesses the greatest refractive density, and which at the same time is not liable to generate air-bubbles. To compensate for the unavoidable loss of light, which by Mr. CANTON and Dr. PRIESTLEY's experiments is found to be considerable in such cases, it perhaps may be necessary to use air achromatic:

achromatic object-glass for the sake of a large aperture, and of such a figure as to shorten the focal distance as much as the observations of such a small angle can admit of. Some contrivance too will be requisite to keep the whole space between the object-glass and the eye-glass always full, notwithstanding the expansions and contractions of the confined fluid by heat and cold, or its waste by evaporation.

It might prove a very considerable abridgement of the necessary apparatus, if this kind of telescope could be connected with the common telescope of a mural quadrant, or zenith sector, and their axes made perfectly parallel by previous observations of a proper terrestrial object. But as there would be some room for apprehending that the exact adjustment of the axes might be affected in raising the telescopes afterwards for celestial observations, this might be examined into by directing them to some star situated in, or very near, the ecliptic, and taking its meridian altitudes at a time of the year when it is in quadrature with the sun, in which case it would have no aberration. But either in this way, or with two separate instruments, the experiment might be made in a few nights, by taking the zenith distance of a proper star, the plane of the instruments being alternately turned different ways in observing, to get the true zenith distance independent of the error of the line of collimation; or the meridian altitude of the pole star may be observed in December above and below the pole, which will give the apparent distance of the star from the pole at that time as affected by aberration. The error of the line of collimation would not affect the result in this way, being the same in the observation both above and below the pole.









**IX. Quantity of Rain which fell at Barrowby near Leeds \*.**  
By George Lloyd, Esq. F. R. S.

Read January 31, 1782.

	1778	1779	1780	1781	Averages.
	Inches.	Inches.	Inches.	Inches.	Inches.
January	2.1	0.6	0.9	1.2	1.2
February	1.3	0.0	1.4	2.7	1.35
March	2.1	0.4	0.5	0.1	0.775
April	0.3	2.0	4.3	1.5	2.025
May	1.8	3.4	1.4	1.1	1.925
June	2.1	2.15	0.8	3.2	2.0625
July	3.9	5.6	1.3	1.6	3.1
August	0.33	2.25	2.45	3.4	2.1075
September	2.87	3.55	4.55	5.6	4.1425
October	5.3	2.8	3.15	0.3	2.8875
November	2.6	2.1	1.95	2.8	2.3625
December	3.3	4.2	0.2	2.1	2.45
Totals	28.0	29.05	22.9	25.6	26.3875

\* See Phil. Trans. vol. LXVII. part I. p. 512.



*X. Account of an improved Thermometer. By Mr. James Six;  
communicated by the Rev. Mr. Wollaston, F. R. S.*

Read February 28, 1782.

**A**TTEMPTING some time ago to ascertain the greatest degree of heat and cold that happened in the atmosphere each day and night, or during the course of twenty-four hours, I experienced the inconvenience which attends thermometers commonly made use of for that purpose; the necessity I mean of the observer's eye being on the instrument the very instant the mercury stands at the highest or lowest degree: for, since the time when that may happen is utterly uncertain, if it be not immediately noticed, it can never after be known. The sultry heat of the summer's days, and freezing cold of the winter's nights, which is commonly most severe at a late unseasonable hour, render it very unpleasant to be abroad in the open air, although it is absolutely necessary for the thermometer to be placed in such a situation. Ingenious men of our own country, as well as foreigners, have, it seems, long ago, endeavoured to remedy this inconvenience; and several thermometers of different constructions have been invented for that purpose. VAN SWINDEN describes one which he says was the first of the kind, made on a plan communicated by Mr. BERNOLLI to Mr. LEIBNITZ. Mr. KRAFT, he also tells us, made one nearly like it\*. A description of those by Lord

\* *Diff. sur la Comparaison du Therm. par VAN SWINDEN, p. 253—255.*

CHARLES

CHARLES CAVENDISH and Mr. FITZGERALD may be seen in the Philosophical Transactions \*. Though much ingenuity appears in the invention of those curious instruments, I could not forbear thinking, that a thermometer might be constructed more conveniently to answer the purpose, and shew accurately the greatest degree of heat and cold which happened in the observer's absence. I therefore attempted to make one: with what success I submit to your better judgement, and proceed to give a description of the instrument. Fig. 1. *ab* is a tube of thin glass, about sixteen inches long, and five sixteenths of an inch in diameter; *cdefgb* a smaller tube with the inner diameter, about one fortieth, joined to the larger at the upper end *b*, and bent down, first on the left side, and then, after descending two inches below *ab*, upwards again on the right, in the several directions *cde*, *fgb*, parallel to, and one inch distant from it. On the end of the same tube at *b*, the inner diameter is enlarged to half an inch from *b* to *i*, which is two inches in length. This glass is filled with highly rectified spirits of wine to within half an inch of the end *i*, excepting that part of the small tube from *d* to *g*, which is filled with mercury. From a view of the instrument in this state, it will readily be conceived, that when the spirit in the large tube, which is the bulb of the thermometer, is expanded by heat, the mercury in the small tube on the left side will be pressed down, and consequently cause that on the right side to rise; on the contrary, when the spirit is condensed by cold, the reverse will happen, the mercury on the left side will rise as that on the right side descends. The scale, therefore, which is FAHRENHEIT's, beginning with 0 at the top of the left side, has the degrees numbered downwards, while that at the right

\* Phil. Transf. vol. L. p. 501. and vol. LI. p. 820.

side, beginning with 0 at the bottom, ascends. The divisions are ascertained by placing this thermometer with a good standard mercurial one in water gradually heating or cooling, and marking the divisions of the new scale at every  $5^{\circ}$  \*. Thus far our thermometer resembles in some respects those of Mr. BERNOULLI and Lord CHARLES CAVENDISH; but the method of shewing how high the mercury had risen in the observer's absence, the essential property of an instrument of this kind, is wholly different from theirs, and effected in the following manner. Within the small tube of the thermometer, above the surface of the mercury on either side, immersed in the spirit of wine, is placed a small index, so fitted as to pass up and down as occasion may require: that surface of the mercury which rises carries up the index with it, which index does not return with the mercury when it descends; but, by remaining fixed, shews distinctly, and very accurately, how high the mercury had risen, and consequently what degree of heat or cold had happened. Fig. 2. represents one of these indexes drawn larger than the real ones, to render it more distinct. *a* is a small glass tube, three quarters of an inch long, hermetically sealed at each end, inclosing a piece of steel wire, nearly of the same length; at each end *cd* is fixed a short piece of a tube of black glass, of such a diameter as to pass freely up and down within the small tube of the thermometer. The lower end, floating on the surface of the mercury, is carried up with it when it rises, while the piece at the upper end, being of the same diameter, keeps the body of the index parallel to the sides of the thermometrical tube. From the upper end of the body of the index at *c* is drawn a spring of glass to

\* The divisions below the freezing point are taken by means of a mixture of sea salt and ice, as described by NOLLET, DE LVC, and others.

the

the fineness of a hair, about five sevenths of an inch in length, which, being set a little oblique, presses lightly against the inner surface of the tube, and prevents the index from following the mercury when it descends, or being moved by the spirit passing up or down, or by any sudden motion given to the instrument by the hand or otherwise; but at the same time the pressure is so adjusted as to permit this index to be readily carried up by the surface of the rising mercury, and downwards whenever the instrument is to be rectified for observation. To prevent the spirit from evaporating, the tube at the end *i* is closely sealed \*. Fig. 3. represents the thermometer on its frame; the plates on which the scale is graved on either side are made to slide out, and the frame is open to the back, behind the large tube, which does not touch it, except at each end. The cap *a*, and the base *b*, are made to fix on with screws, and only cover the turning of the small tube. By a screw at the bottom of the frame, it may be made fast to the wall against which it is to hang without doors, to prevent its being shaken by violent winds. Towards evening I usually visit my thermometer, and see at one view, by the index on the left side, the cold of the preceding night; and by that on the right, the heat of the day. These I enquire down, and then apply a small magnet to that part of the tube against which the indexes rest, and move each of them down to the surface of the mercury: thus, without heating, cooling, separating, or at all disturbing the mercury, or moving the instrument, may this

\* When this tube is closed (not hermetically, but only so as to prevent the spirits evaporating) the thermometer must be brought to the greatest heat it is likely at any time after to sustain; and though no more air is inclosed than what remains at that time above the spirits, yet that will, by its elasticity pressing on the fluid, answer every purpose as well as if the external air was freely admitted.

thermometer, without a touch, be immediately rectified for another observation. When I wish to put the thermometer out of my hand, without hanging it up, I have a stand to place it on; for if the mercury presses against the index, while the instrument lies in an horizontal position, it is in danger of passing by it, which is avoided by keeping the thermometer in a position nearly vertical. To prevent the mercury shifting its place in the spirits within the tube (which I apprehended it might do on account of the superiority of its specific gravity, especially when kept for a considerable time, very high on one side, and low on the other), I made that part of the small tube from *e* to *f* with the inner diameter exceeding small; and found, upon trial, that after the summer's heat had kept the mercury for a long time high on one side, the winter's cold brought it again as accurately to the freezing point on the other as at first\*. This thermometer may be made a mercurial one by inverting the glass, and filling with mercury that part which in the first is filled with spirits, and with spirits that part of the small tube from *d* to *g* which in the former is filled with mercury; the indexes in either case may be the same, and will be carried up in the same manner upon the surface of the mercury; but the end of the tube at *i*, instead of being sealed, must then be left open, and stand inverted in a bulb, or small cistern of mercury, into which the external air has free access. The diameter of the tube *ab* should be considerably increased if the degrees on the scale are required to be as wide as those in the spirit thermometers. It is indeed better in this case to have a double rather than a larger single tube; but finding the weight of so great a quantity of mercury in a thin glass tube

\* With a thermometer of this sort I observed the greatest heat and cold that happened every day and night throughout the year 1781.

attended with many disadvantages, and the motion of the fluids in the spirit-ones perfectly agreeing with, and being as readily excited by change of heat and cold, as in the mercurial thermometers, I preferred the former as much more commodious. A person cannot approach near to the thermometer first described when the air is very cold (especially with a light which by night is necessary) without causing the spirits presently to expand, and consequently the mercury on the left side immediately to descend. This sensibility is here attended with every advantage, without the inconvenience to which common thermometers in this case are liable \*; for the index will accurately shew the greatest height to which the mercury had risen, although, before the exact degree can well be distinguished, it will appear separated from the index, and descending apace. As the scale is sixteen inches long, and divided into 100° only, which are more than sufficient for the temperature of the air, they are large enough to be sub-divided at pleasure. The indexes, though of a tender and delicate nature, when once placed in the tube, are not liable to suffer any alteration by time or accident; and the thermometer may be exposed to rain at all times, without suffering the least injury in any respect.

In constructing the thermometer before mentioned, I at first hit on a plan by which the same end was obtained by a dif-

\* The most sensible mercurial thermometers commonly have the column of mercury as well as the degrees very small, and a person assisted with a light can hardly view them near enough, when the weather is very cold, without causing the mercury to rise before the degrees where it stood can be well ascertained.

Freezing fogs also, which with us usually attend the greatest degrees of cold, by covering the glass with frost, render the mercury invisible, and cannot well be removed without causing the to rise, or at least render the observation doubtful, which at such a time is very disagreeable; for, in proportion to the extraordinary degree of cold, so is our curiosity likely to be excited.

ferent



ferent method; and though, in some respects, and for some purposes, it may not be so proper as that already described, yet, for some others, it may be found useful, and therefore I shall briefly describe it. The glass of this instrument is in all respects the same as in the former, excepting that the diameters of the tubes are something larger. It is likewise filled with spirits of wine and mercury, in the same manner; but the indexes are different, being only a small tube of black glass, about five-sevenths of an inch in length, hermetically sealed at each end, containing a piece of steel wire. An index of this sort is placed in the thermometer on either side, which, having no spring to support them, sink down in the spirits, and rest upon the mercury. Whenever the mercury descends, the index will follow it; but when it rises, the index will not rise with it, and by remaining at the place to which the mercury had descended, will shew the greatest degree of heat or cold which had happened. In this manner do these indexes answer the same purpose, though they move directly contrary to the others in the other thermometer; but this instrument is not so easily rectified as the former, for the most powerful magnet will not bring the index up again while the mercury above presses against them; and although it is possible to remove the mercury, and by that means set the index at liberty, yet inconveniences will be incurred from which the other is entirely free.

In some cases it may be found expedient, instead of the double thermometer first described, to make two single ones; one to shew the greatest degree of heat only, and the other the cold, each having its proper index (see fig. 4. and 5.). The first has the small tube bent down on the left side, and the lower end immersed in a bulb or small cistern of mercury, to which the external air has free access; the other has the small tube

tube turned up on the right side, with some mercury let down to the bottom, and the upper end closely sealed, as in the double instrument. Making a standard mercurial thermometer, by which the scale of the spirit one was to be divided, I endeavoured to obtain as wide degrees as possible, that the motion of the mercury might thereby be rendered more conspicuous, and the height of it ascertained with greater precision. It is true, the larger the degrees, the larger in some measure must be the bulb, and therefore the fluid contained in it not likely to be so soon affected by any change of heat or cold in the atmosphere as in a smaller. But as this thermometer was principally to be used immersed in a large quantity of water, gradually heating or cooling, little or no disadvantage could arise from making the bulb somewhat larger than those commonly made use of in the air. Not being able, however, to procure glass-tubes so long as I had occasion for, whose inner diameters were perfectly equal, I took the following method to adjust the divisions on the scale to the inequality of the tubes. Choosing a tube of a length suitable to my purpose, with a proper bulb at the end, I put into it a small quantity of mercury \* sufficient to form a column about one inch in length. Drawing then on a board the three lines *aa*, *bb*, *cc*, fig. 6. I placed the glass-tube on the line *aa*, and while the mercury remained at rest at the end of the tube, near the bulb, I made two pencil marks on the line *aa*, one at *d*, and the other at *e*, perfectly coin-

\* To put in a small quantity of mercury, and measure its length at different parts of the tube, as described by Abbé NOLLET, vol. IV. p. 370. Leçons Physique, is a very excellent method to discover the error; but in what manner readily to adjust the scale, so as to avoid any inaccuracy from such inequality (which in tubes of the length I had occasion for seemed to me unavoidable) was a matter concerning which I could meet with no information.

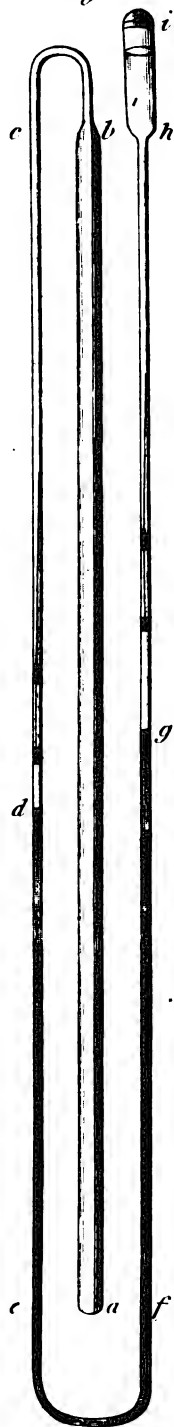
ending

ciding with the two ends of the column of mercury: then causing the mercury to move slowly on farther from the bulb, till that end of the column which was first at *d* coincided with the mark at *e*, and letting it rest again, I made another mark at *f*; after which, causing the mercury to move on as before, and continuing to mark its length at every part of the tube till it reached the end farthest from the bulb; by these means I obtained the several intermediate points on the line *aa*. Through these several points I drew dotted lines parallel to each other, and at right angles with the line *aa* to the line *bb*. Taking now, with a pair of compasses, the widest intervals between any of the dotted parallels, which in this case is from *d* to *e*, I inferted that distance successively between the several parallels, beginning at the lowest pair, as from *d* to *e*, from *e* to *f*, from *f* to *g*, and so on to *h*, as exhibited in the figure; and the aggregate of these lines may be considered as one continued line, without any error of consequence in this matter. Having now the thermometer completely filled with mercury, the air expelled, the point of the scale at  $102^{\circ}$ , and the freezing point properly taken \* and marked upon the tube, which was now hermetically sealed, I again applied the tube to the line *aa*; and marked on that line the point of  $102^{\circ}$  and the freezing point. Through those points I drew the lines *ii*, *kk*, and divided that part of the compound line *db* included between *ii*

\* The freezing point, marked on the tube of this thermometer, is immediately taken by means of grated ice; but the point of  $100^{\circ}$  by a standard mercurial thermometer, the upper point of the scale of which was properly taken by boiling water, and the lower one by grated ice; but it is more commodious in the first to have the tube no longer than the air scale, especially as the degrees are pretty wide. The method of adjusting the scale to the inequality of the tube remains the same, let the given points be at any distance, or the divisions increased to any number.

and

*Fig. 1*



*Fig. 2*





and *kk* into 14 equal parts, beginning at *o*, the point where *ii* cuts the line *dh*, continuing afterward six divisions now on that line below *kk*, making in all 20 equal divisions. If now lines be drawn through each of the dividing points, from *o* to 20 to the line *cc*, at right angles with the same, they will give on the line *cc* the true thermometrical scale to every  $5^{\circ}$  from 2 to 102, properly adjusted to the inequality of the tube\*, which in this case is nearly of the same diameter at each end, but smaller towards the middle. Tubes may indeed be found of some considerable length with less inequality than what this scale exhibits; but the error is here enlarged, to render the method of correcting it more conspicuous.

\* Experimentally to prove this method I have made mercurial thermometers, whose scales from the freezing point to that of boiling heat were nearly three feet, and though the inequalities of the tubes were very considerable, varying in contrary directions to each other; yet when they were placed on the same frame, they perfectly agreed in a motion of the mercury in every part of their scales.

XI. *On the Parallax of the Fixed Stars.* By Mr. Herschel,  
F. R. S.; communicated by Sir Joseph Banks, Bart. P. R. S.

Read December 6, 1781.

TO find the distance of the fixed stars has been a problem which many eminent astronomers have attempted to solve; but about which, after all, we remain in a great measure still in the dark. Various methods have been pursued without success, and the result of the finest observations has hardly given us more than a distant approximation, from which we may conclude, that the nearest of the fixed stars cannot be less than forty thousand diameters of the whole annual orbit of the earth distant from us. Trigonometry, by whose powerful assistance the mathematician has boldly ascended into the planetary regions, and measured the diameters and orbits of the heavenly bodies, for want of a proper base, can here be but of little service; for the whole diameter of the annual orbit of the earth is a mere point when compared to the immense distance of the stars. Now, as it is not in our power to enlarge this base, we can only endeavour to improve the instruments by which we measure its parallax.

There are two things requisite for measuring extremely small angles with accuracy. First, that the instrument we use for this purpose, be it quadrant, sector, or micrometer, should be divided and executed with sufficient exactness; and, secondly, that the telescope, by which the observations are to be made,

made, should have an adequate power and distinctness. Upon the first head, the great improvements of mathematical instrument-makers have hardly left us any thing to desire: we can now measure seconds with almost as much facility and truth as former observers could measure minutes; nor do I think it impossible to go still further, and divide instruments that would shew thirds with sufficient accuracy. It is in the latter, or optical part, we find the greatest difficulty. To see a single second of a degree with precision requires a telescope of very great perfection; therefore, supposing the mechanical part of an apparatus well executed, it will still be necessary to try how far the power of our telescope will enable us to ascertain with confidence the division or number of seconds it points out. If upon trial we find that our instrument will give us the same measure within the second, every time the experiment is repeated, we may pronounce it capable of measuring seconds; if otherwise, it will remain to be examined, whether the fault lies in the mechanical or optical part.

Let us now suppose that the parallax of the fixed stars does not amount to a single second, yet still the case is by no means desperate; and though the difficulty of measuring seconds will soon suggest to us what extraordinary powers and distinctness of the telescope, and accuracy of the micrometer, are required to measure thirds; this ought by no means to discourage us in the attempt. Could we measure angles, much smaller than seconds, might we not hope to find the parallax of some of the fixed stars at least to amount to several thirds? On the other hand, if it should appear, indeed, that even with such improved methods of measurement we could not reach the remote situation of such almost infinitely distant suns, we might still derive a valuable approximation towards truth from



such repeated observations, even though they should not be attended with all the success we expected from them. On this assurance, I endeavoured to take such a method for attempting the investigation of the parallax of the stars as to avail myself of the improvements I had already made, and was still in hopes of making, in my telescopes.

The next thing that was necessary to consider in this undertaking was, the manner of putting it into execution. The method pointed out by GALILEO, and first attempted by HOOK, FLAMSTEAD, MOLINEUX, and BRADLEY, of taking distances of stars from the zenith that pass very near it, though it failed with regard to parallax, has been productive of the most noble discoveries of another nature. At the same time it has given us a much juster idea of the immense distance of the stars, and furnished us with an approximation to the knowledge of their parallax that is much nearer the truth than we ever had before. Dr. BRADLEY, in a letter to Dr. HALLEY on the subject of a new discovered motion of the fixed stars, says, “ I believe I may venture to say, that in either of the “ two stars last mentioned ( $\gamma$  Draconis and  $\eta$  Ursæ majoris) it “ (the annual parallax) does not amount to 2”. I am of opinion, that if it were 1” I should have perceived it in the “ great number of observations that I made, especially upon “  $\gamma$  Draconis ; which agreeing with the hypothesis (without “ allowing any thing for parallax) nearly as well when the “ sun was in conjunction with, as in opposition to, this star, “ it seems very probable, that the parallax of it is not so great “ as one single second.” *Phil. Trans.* n. 406. p. 637. Dec. 1728. As I do not know that any thing more decisive has been done upon the subject, it will not be amiss to see how far this method of finding the parallax has really been successful.

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The instrument that was used upon this occasion was the same as the present zenith sectors, which can hardly be allowed sufficient to shew an angle of one or even two seconds with accuracy; yet, on account of the great number of observations, and above all the great sagacity of the observer, we will admit that if the parallax had amounted to two seconds he would have perceived it. The star on which these observations were made is marked of the third magnitude in the catalogue of PTOLEMY; in TYCHO BRAHE's of the third; in the Prince of HESSE's of the third; in HEVELIUS's between the third and second; in FLAMSTEAD's of the second; and now appears as a very bright star of the third, or small star of the second magnitude; therefore its parallax is probably considerably less than that of a star of the first magnitude. Several authors who have touched upon this subject seem to have overlooked this distinction; and from Dr. BRADLEY's account of the parallax of  $\gamma$  Draconis, have concluded the parallax of the stars in general not to exceed  $1''$ ; but this appears to me by no means to follow from the doctor's observations. It is rather evident that, for aught we know to the contrary, the stars of the first magnitude may still have a parallax of several seconds; and I believe this to be as accurate a result as that method is capable of giving, at least in latitudes where there is not a star of the first magnitude that passes directly through the zenith\*.

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\* DE LA LANDE, in his excellent book of *Astronomy*, says, that the parallax of the fixed stars has been proved to be absolutely insensible (*Ast. liv. XVI. § 2782.*). He reports the observations of TYCHO BRAHE, PICARD, HOOK, and FLAMSTEAD, and concludes (§ 2778.) from the discovery of the aberration by Dr. BRADLEY (which it seems he also allows to be the most decisive upon the subject) that now the question about parallax is resolved. In giving us the opinion which  
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In general, the method of zenith distances labours under the following considerable difficulties. In the first place, all these distances, though they should not exceed a few degrees, are liable to refractions; and I hope to be pardoned when I say that the real quantities of these refractions, and their differences,

the doctor had of the result of his own observations with regard to the annual parallax, DE LA LANDE only mentions "M. BRADLEY pense que si elle (la "parallaxe) eût été seulement de 1'' il l'auroit apperçue dans le grand "nombre d'observations qu'il avoit faites, surtout de  $\gamma$  du Dragon." But if we also take in those lines upon which Dr. BRADLEY seems to lay the greatest stress, viz. "I believe I may venture to say, that in either of the "two stars last mentioned it does not amount to two seconds;" and if we allow for the magnitude of the stars upon which the observations were made, I think I have fairly stated the full amount of all the actual proofs we have of the smallness of the annual parallax. Now, since it has escaped the finest observations of BRADLEY, it is not likely that it should come up to the full quantity to which it might amount without being perceived; and therefore the doctor might think it highly probable, "that it is not so great as one single second;" and his opinion, as well as DE LA LANDE's, who believes it to be absolutely insensible, are perfectly consistent with all the observations that have hitherto been made; though the *actual proofs*, which are the subject of our present inquiry, do not extend so far. Against the parallax of Sirius DE LA LANDE (§ 2781.) mentions "forty- "five meridian altitudes taken by Dr. BEVIS[a], with the eight-feet mural qua- "drant of the Royal Observatory at Greenwich, none of which differed 3 or 4'' "from the mean altitude." Now, if they differed 3 or 4'' from the mean, we may suppose they differed 6 or 8'' from each other; and that observations, subject to so many causes of error as I shall presently enumerate, and which differed so much from each other, cannot give the least evidence either for or against a parallax, will need no proof. Refraction alone, which is liable to such changes at the meridian altitude of Sirius, notwithstanding the most careful observations of the barometer and thermometer should be made to ascertain its quantity, would, with me, remain an unanswerable argument against the validity of such observations in a subject of this critical nicety.

[a] These observations were not made by Dr. BEVIS, but extracted from the registers of the Royal Observatory at my desire, and calculated by myself, and sent in a letter by Dr. BEVIS to Paris.

are very far from being perfectly known. Secondly, the change of position of the earth's axis arising from nutation, precession of the equinoxes, and other causes, is so far from being completely settled, that it would not be very easy to say what it exactly is at any given time. In the third place, the aberration of light, though best known of all, may also be liable to some small errors, since the observations from which it was deduced laboured under all the foregoing difficulties. I do not mean to say, that our theories of all these causes of error are defective; on the contrary, I grant that we are for most astronomical purposes sufficiently furnished with excellent tables to correct our observations from the above mentioned errors. But when we are upon so delicate a point as the parallax of the stars; when we are investigating angles that may, perhaps, not amount to a single second, we must endeavour to keep clear of every possibility of being involved in uncertainties; even the hundredth part of a second becomes a quantity to be taken into consideration.

I shall now deliver the method I have taken, and shew that it is free from every error to which the former is liable, and is still capable of every improvement the telescope and mechanism of micrometers can furnish.

Let OE (fig. 1.) be two opposite points of the annual orbit, taken in the same plane with two stars  $a$ ,  $b$ , of unequal magnitudes. Let the angle  $aOb$  be observed when the earth is at O: and let the angle  $aEb$  be also observed when the earth is at E. From the difference of these angles, if any should be found, we may calculate the parallax of the stars, according to a theory that will be delivered hereafter. These two stars, for reasons that will soon appear, ought to be as near each other as possible,

possible, and also to differ as much in magnitude as we can find them.

GALILEO, I believe, was the first who suggested this method; but in the manner he mentions it in his third dialogue of the *Systema Cosmicum*, it would be exposed to all the difficulties we have enumerated, and would wish to avoid; for he does not observe, that the two stars should be so near each other as thereby to preclude the influence of every cause of error.

This method has also been mentioned by other authors; and we find that Dr. LONG observed the double star which is the first of Aries in PTOLEMY's catalogue; that in the head of Castor; the middle one in the sword of Orion; and that in the breast of Virgo, with telescopes of fourteen and seventeen feet, and "was persuaded they would be found always to appear the same." But when the theory of parallax will be explained, it will be seen that every one of these stars are totally improper for the purpose; for the stars of  $\gamma$  Arietis are near 10" distant from each other, and moreover equal in magnitude. In  $\alpha$  Geminorum the stars, though near enough, do not sufficiently differ in magnitude to shew any parallax. The stars in the Nebula of Orion, on account of their extreme smallness or distance, are still more improper than any; and those of  $\gamma$  Virginis are equal in magnitude.

I do not find that any thing else has been done upon the subject. GALILEO justly remarks, that such observations ought to be made with the best telescopes, and upon this occasion mentions the power of his own, which enlarged the disk of the sun a thousand times, from which we find it magnified about thirty-two times; but we can hardly think his nor even Dr. LONG's, whose power might probably be sixty or seventy, sufficient for the purpose. What would GALILEO say, if he were told that

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our present opticians make instruments that enlarge the disk of the sun above forty thousand times? What would even CASSINI say, if he were to view the first star of Aries, which appeared to him as split in two, through a telescope that will shew  $\eta$  Coronæ borealis and  $\beta$  Draconis to be double stars?

But to proceed, I shall now prove that this method, if stars properly situated (such as I have found) are taken, is free from all the errors occasioned by refraction, nutation, precession of the equinoxes, changes of the obliquity of the ecliptic, and aberration of light; and that the annual parallax, if it even should not exceed the tenth part of a second, may still become visible, and be ascertained at least to a much greater degree of approximation than it ever has been done.

It will also appear, from the great number of observations I have already made upon several double stars, especially  $\epsilon$  Bootis, that we can now with much greater certainty affirm the annual parallax to be exceedingly small indeed; and that there is a great probability of succeeding still farther in this laborious but delightful research, so as to be able at last to say, not only how much the annual parallax *is not*, but how much it really *is*.

Let there be two stars at a distance from each other, not exceeding five seconds; suppose them to be observed at an altitude of  $20^\circ$ ; and let them be so situated with respect to each other, that one of them may be  $20''$ , and the other  $20''$  and  $5''$  high: then the whole effect of mean refraction at that altitude, by Dr. MASKELYNE's excellent tables, will be  $2' 35''.5$  for  $20^\circ$ , and  $2' 35''.4888$  for  $20^\circ 5''$ . The difference is  $0''.0111$ . Now, in the first place, we have nothing to do with the refraction itself, since the real altitude of the stars is not in question. In the next place, we also have no concern with the difference of refraction

tion between the two stars, though no more than the ,0111th part of a second, because the real distance between the two stars is not required. It follows then, that these observations can only be affected by the difference of the difference; that is, by an alteration in the quantity of refraction occasioned by the change of heat and cold, or weight of the atmosphere, and pointed out to us by the rise and fall of the barometer and thermometer. Let us then see what this difference of the difference may amount to. Suppose a change of  $22^{\circ}$  of FAHRENHEIT's thermometer, that is, from the freezing point to the moderate air of a summer's night, and a difference of an inch in the height of the barometer; these two causes both conspiring, which does not often happen, may occasion an alteration of ,00096th part of a second in five, at an altitude of  $20^{\circ}$ ; but this being less than the thousandth part of a second may safely be rejected as a quantity altogether insensible.

Since it may not be always convenient to view these stars at the altitude of  $20^{\circ}$ , it remains to see what effect different altitudes may have: let us then make the most unfavourable supposition, that they may one time be seen in a horizontal position, having before been seen vertical. In this case, as the whole difference of refraction in a difference of  $5''$  of altitude is no more than ,0111, provided they are observed not lower than  $20^{\circ}$ , and the whole difference of the difference of refraction is only ,0009; the sum ,012, when both conspire, not exceeding much the hundredth part of a second, may still be rejected as insensible. Let us also examine how near the horizon it may be safe to observe such stars. At  $10^{\circ}$ , for instance, the refraction is  $5' 14'' ,6$ ; the difference for  $5''$  is ,0388; the joint effect of the changes in the barometer and thermometer is ,0034; the sum of the whole together amounts to ,0422, which

is less than half the tenth of a second: now this may either be taken into consideration, or such low observations may be avoided, as being by no means necessary, and but ill suiting the high powers a telescope proper for this purpose ought to bear.

The change of position of the earth's axis I look upon as an unsurmountable obstacle to taking the parallax of stars by the method of zenith distances: for though refraction is much reduced in the zenith, this change is there no less sensible than in other parts of the heavens; but as this will always affect our two stars exactly alike, we are entirely freed from this embarrassment.

The aberration of light can have no influence of the least consideration upon our two stars, as a mere inspection of the tables will shew. In a whole degree, its effects, when greatest, amount but to four-tenths of a second, and consequently in 5" to no more than ,0005, or the two thousandth part of a second.

Observations of the relative distance of the two stars that make up a double star, being thus cleared of every impediment, are capable of being continually improved by every degree of perfection the telescope may acquire: we can chuse stars that may be viewed sufficiently high to be clear of the vapours that swim near the horizon, and consequently employ the greatest powers our instruments are capable of. From experience I can also affirm, that the stars will bear a much higher degree of magnifying than other celestial objects. Too much has hitherto been taken for granted in optics: every natural philosopher is ready enough to allow the necessity of making experiments, and tracing out the steps of nature; why this method should not be more pursued in the art of seeing



does not appear. Theories are only to be used when proper data are assigned; but the data are carefully to be re-examined, when new improvements may widely alter the result of former experiments. Thus, we are told, that we gain nothing by magnifying *too much*. I grant it; but shall never believe I magnify too much till by experience I find, that I can see better with a lower power. Nor is even that sufficient: a lower power may shew more of the object; it may shew it brighter, may even distincter, and therefore upon the whole better; and yet the greater power may, in a particular case, be preferable: for if the object is so small as not to be at all visible with the lower power, and I can, by magnifying more, obtain a view of it, though neither so bright nor distinct as I could wish, is it not evident, that here this power is preferable to the former?

The naturalist does not think himself obliged to account for all the phænomena he may observe; the astronomer and optician may claim the same privilege. When we increase the power we lessen the light in the inverse ratio of the square of the power; and telescopes will, in general, discover more small stars the more light they collect; yet with a power of 227 I cannot see the small star near the star following  $\alpha$  Aquilæ, when, by the same telescope, it appears very plainly with the power of 460: now, in the latter case, the power being more than double, the light is less than the fourth part of the former. In such particular cases I generally suspect my own eyes, and have recourse to those of my friends. I had the pleasure of shewing this star to Dr. WATSON junior, who soon discovered the small star, which accompanies the other, with the power of 460; but saw nothing of it with 227, though the place where to look for it had been pointed out to him by the higher power. The experiment has been too often repeated

repeated to be doubtful, and has also been confirmed by others of nearly the same nature: for instance, the smallest of the two that accompany the star near  $\kappa$  Aquilæ, the small star near  $\mu$  Herculis, and the small star near  $\alpha$  Lyræ, are invisible with my power of 227, and visible with the same aperture when the power is 460. Also the small stars near FLAMSTEAD'S 24th of Aquila, the smallest of two near  $\sigma$  Coronæ, the small star near the star south of  $\epsilon$  Aquilæ, the small star near the second  $\delta$  Persei, the small star near the star which accompanies FLAMSTEAD'S 10th sub pede et scapula dextra Tauri, the small star, near  $\beta$  Delphini, and the small star near the pole star, are all much brighter and stronger, and therefore much sooner seen with 460 than with 227.

Great power may also, in particular circumstances, be favourable, even with an excess of aberration. When two stars are so close together as to make the scale for measuring the distance of their centers too small, if, by magnifying much, we can enlarge that distance, we may gain a considerable advantage, provided the centers or apparent bodies of the stars remain distinct enough for the purpose of these measures. The appearance of  $\alpha$  Lyræ in my Newtonian reflector with a power of 460 is represented in fig. 2.; with 2010 in fig. 3.; with 3168 in fig. 4.; and with 6450 in fig. 5. Now in all these figures we see, that the centers are still distinct enough to measure their distances with sufficient truth; or if any little error should be introduced by the magnitude of the central point, it will be more than sufficiently balanced by the largeness of the scale. In this manner, with a power of 3168, I have obtained a scale of no less than ten inches six tenths for the distance of the centers of the two stars of  $\alpha$  Geminorum; and as we

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know these centers to be but a few seconds distant, it is plain how great an advantage we gain by such an enlarged scale.

These experiments have but very lately pointed out to me a method of making a new micrometer, upon a construction entirely different from any that are now in use, which I have been successful enough to put in practice, and by which I have already begun to determine the distance of the centers of some of the most remarkable double stars to a very great degree of accuracy\*.

The powers that may be used upon various double stars are different, according to their relative magnitudes:  $\alpha$  Bootis, for instance, will not bear the same power as  $\alpha$  Geminorum, nor would it be difficult to assign a reason for it; but as I here shall merely confine myself to facts, it will be sufficient in general to mention, that two stars, which are equal, or nearly so, will bear a very high power: with  $\alpha$  Geminorum I have gone as far as 3168; but with the former only to 2010. The difficulty of using high powers is exceedingly great; for the field of view takes in less than the diameter of the hair or wire in the finder, and the effect of the earth's diurnal motion is so great, that it requires a great deal of practice to find the object, and manage the instrument. It appears to me very probable, that the diurnal motion of the earth will be the greatest obstacle to our progress in magnifying, except we can introduce a proper mechanism to carry our telescopes in a contrary motion.

Notwithstanding opticians have proved that two eye-glasses will give a more correct image than one, I have always (from experience) persisted in refusing the assistance of a second glass, which is sure to introduce errors greater than those we would correct. Let us resign the double eye-glass to those who view objects

\* For a description of this micrometer see a subsequent paper.

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merely for entertainment, and must have an exorbitant field of view. To a philosopher this is an unpardonable indulgence. I have tried both the single and double eye-glasses of equal powers, and always found that the single eye-glass had much the superiority in point of light and distinctness. With the double eye-glass I could not see the *belts on Saturn*, which I very plainly saw with the single one. I would, however, except all those cases where a large field is absolutely necessary, and where power joined to distinctness is not the sole object of our view.

The application of the different powers of a telescope in general is of some consequence; and in answer to those who may think I have strained or over-charged mine, I must observe, that a single glance at the subsequent *b Draconis*,  $\gamma$  *Coronæ*, and the star near  $\mu$  *Boötis*, with a power of 460, shewed them to me as double stars; when, in two former reviews of the heavens, I had twice set them down in my journal as single stars, where I used only the power of 222 and 227, and in all probability should never have found them double, had I not looked with a higher power.

We are to remember, that it is much easier to see an object when it is pointed out to us than when it falls in our way unexpectedly, especially if of such a nature as to require some attention to be seen at all; but to say no more of other advantages of high powers, it is evident, that in the research of the parallax of the fixed stars they are absolutely necessary. If we would distinctly perceive and measure or estimate extremely small quantities, such as a tenth of a second, it appears, that when we use a power of 460, this tenth of a second will be no more in appearance than  $46''$ , and even with a power of 1500 will be but  $2' 30''$ , which is a quantity not much more than

than sufficient to judge well of objects and distinguish them from each other, such as a circle from a square, triangle, or polygon\*.

It has been observed, that objects grow indistinct when the principal optic pencil at the eye becomes less than the 40th or 50th part of an inch in diameter. In the experiments that have been made upon this subject it appears to me, that the indistinctness which is ascribed to the smallness of the optical pencil may be owing to very different causes: at least it will be easy to bring contrary experiments of extremely small pencils, not at all affected by this inconvenience; for instance, it is well known, that microscopes, consisting of a single lens or globule, are remarkable for distinctness. We also know, that they have been made so small as to magnify above 10,000 times†. From this we may infer that their apertures, and consequently the diameters of the optic pencil at the eye could not exceed the 2500th part of an inch. I am therefore inclined to believe, that we must look for distinctness in the perfection of the object-speculum or object-glass of a telescope; and if we can make the first image in the focus of a speculum almost as perfect as the real object, what should hinder our magnifying but the want of light? Now, if the object has light sufficient, as the stars most undoubtedly have, I see no reason why we should limit the powers of our instruments by any theory. Is it not best to have recourse to experiments to find

\* By a set of experiments, made in the year 1774, I found, that I could discover or perceive a bright object, such as white paper, against the sky-light, when it subtended an angle of  $35''$ ; but could only distinguish it to be a circle, and no other figure, when it appeared under an angle of  $2' 24''$ .

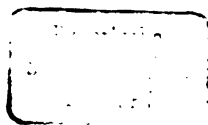
† See Padre DELLA TORRE's Method, &c. *Scelta di Opuscoli*.

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how far our endeavours to render the first image perfect have been successful.

As soon as I was fully satisfied that in the investigation of parallax the method of double stars would have many advantages above any other, it became necessary to look out for proper stars. This introduced a new series of observations. I resolved to examine every star in the heavens with the utmost attention and a very high power, that I might collect such materials for this research as would enable me to fix my observations upon those that would best answer my end. The subject has already proved so extensive, and still promises so rich a harvest to those who are inclined to be diligent in the pursuit, that I cannot help inviting every lover of astronomy to join with me in observations that must inevitably lead to new discoveries. I took some pains to find out what double stars had been recorded by astronomers; but my situation permitted me not to consult extensive libraries, nor indeed was it very material: for as I intended to view the heavens myself, Nature, that great volume, appeared to me to contain the best catalogue upon this occasion. However, I remembered that the star in the head of Castor, that in the breast of the Virgin, and the first star in Aries, had been mentioned by CASSINI as double stars. I also found the Nebula in Orion was marked in HUGEN'S *Systema Saturnium* as containing seven stars, three of which (now known to be four) are very near together. With this small stock I begun, and in the course of a few years observations have collected the stars contained in my catalogue. I find, with great pleasure, that a very excellent observer, whom I have the honour to call my friend\*, has also, though un-

\* Phil. Transf. for the year 1781, part II. double stars discovered in 1779, at Frampton-house, Glamorganhire, by NAT. PIGOTT, Esq. F. R. S. &c.



known to me, met with three of those stars that will be found in my catalogue: and upon this occasion I also beg leave to observe, that the Astronomer Royal, when I was at Greenwich last May, with his usual politeness, shewed me, among other objects,  $\alpha$  Herculis as a double star, which he had discovered some years ago. The rev. Mr. HORNSBY also, when I had the pleasure of seeing him at Oxford, in a conversation on the subject of the stars of the first magnitude that have a proper motion, mentioned  $\pi$  Bootis as a double star. It is a little hard upon young astronomers to be obliged to discover *over-again* what has already been discovered; however, the pleasure that attended the view when I first saw these stars has made some amends for not knowing they had been seen before me.

If I should mention in my list of observations a few that may be found difficult to be verified by other telescopes, I must beg the indulgence of the observers. I hope it will sufficiently appear, that I have guarded against optical delusions; and every astronomer, I make no doubt, will find, by those observations that fall within the compass of his instruments, and attention to circumstances necessary to the right management of them, that I have had all along truth and reality in view, as the sole object of my endeavours; and therefore he will be inclined to give some credit to what he does not immediately perceive, when he finds himself successful where he takes the proper precautions so necessary in delicate observations, even with the best instruments.

I have been in some doubt in what manner to communicate these observations. My first view was to have methodized them properly; but I find them so extensive that there is but little probability that one person should be able to bring them to a conclusion, for which reason I have now resolved to give them

unfinished as they are, that every person who is inclined to engage in this pursuit may become a fellow-labourer.

In settling the distances of double stars I have occasionally used two different ways. Those that are extremely near each other may be estimated by the eye, in measures of their own apparent diameters. For this purpose their distance should not much exceed two diameters of the largest, as the eye cannot so well make a good estimation when the interval between them is greater. This method has often the preference to that of the micrometer: for instance, when the diameter of a small star, perhaps not equal to half a second, is double the vacancy between the two stars. Here a micrometer ought to measure tenths of seconds at least, otherwise we could not, with any degree of confidence, rely on its measures; nay, even then, if the stars are situated in the same parallel of declination and near the equator, their quick motion across the micrometer makes it extremely difficult to measure them, and in that case an estimation by the eye is preferable to any other measure; but this requires not a little practice, precaution, and time, and yet with proper care it will be found that this method is capable of great exactness. Let two small circles be drawn either equal or unequal, at a distance not exceeding twice the diameter of the largest; let these be shewn to several persons in the same light and point of view. Then, if every one of them will separately and carefully write down his estimation of the interval between them, in the proportion of either of their diameters, it will be found upon a comparison that there will seldom be so much as a quarter of a diameter difference between all the estimations. If this agreement takes place with so many different eyes, much more may we expect it in the

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estimations



estimations of the same eye when accustomed to this kind of judgement.

I have divided the double stars into several different classes. In the first I have placed all those which require indeed a very superior telescope, the utmost clearness of air, and every other favourable circumstance to be seen at all, or well enough to judge of them. They seemed to me on that account to deserve a separate place, that an observer might not condemn his instrument or his eye if he should not be successful in distinguishing them.

As these are some of the finest, most minute, and most delicate objects of vision I ever beheld, I shall be happy to hear that my observations have been verified by other persons, which I make no doubt the curious in astronomy will soon undertake. I should observe, that since it will require no common stretch of power and distinctness to see these double stars, it will therefore not be amiss to go gradually through a few preparatory steps of vision, such as the following: when  $\eta$  Coronæ borealis (one of the most minute double stars) is proposed to be viewed, let the telescope be some time before directed to  $\alpha$  Geminorum, or if not in view to either of the following stars,  $\zeta$  Aquarii,  $\mu$  Draconis,  $\rho$  Herculis,  $\alpha$  Piscium, or the curious double-double star  $\epsilon$  Lyræ. These should be kept in view for a considerable time, that the eye may acquire the habit of seeing such objects well and distinctly. The observer may next proceed to  $\xi$  Ursæ majoris, and the beautiful treble star in Monoceros's right fore-foot; after these to  $i$  Bootis, which is a fine miniature of  $\alpha$  Geminorum, to the star preceding  $\alpha$  Orionis, and to  $\pi$  Orionis. By this time both the eye and the telescope will be prepared for a still finer picture, which is  $\eta$  Coronæ borealis. It will be in vain to attempt this latter if all the former, at least

*i*-Bootis, cannot be distinctly perceived to be fairly separated because it is almost as fine a miniature of *i* Bootis as that is of  $\alpha$  Geminorum. If the observer has been successful in all these, he may then, at the same time, try *b* Draconis, though I question whether any power less than 4 or 500 will shew it to be double; but the former I have all seen very well with 227.

To try the stars of unequal magnitudes it will be expedient to take them in some such order as the following:  $\alpha$  Herculis,  $\omega$  Aurigæ,  $\delta$  Geminorum,  $k$  Cygni,  $\epsilon$  Persei, and *b* Draconis; from these the observer may proceed to a most beautiful object,  $\epsilon$  Bootis, which I have closely attended these two years as very proper for the investigation of the parallax of the fixed stars.

It appears, from what has been said, that these double stars are a most excellent way of trying a telescope; and as the foregoing remarks have suggested the method of seeing how far the power and distinctness of our instruments will reach, I shall add the way of finding how much light we have. The observer may begin with the pole-star and  $\alpha$  Lyrae; then go to the star south of  $\epsilon$  Aquilæ, the treble star near  $k$  Aquilæ, and last of all to the star following  $\sigma$  Aquilæ. Now, if his telescope has not a great deal of good distinct light, he will not be able to see some of the small stars that accompany them.

In the second class of double stars I have put all those that are proper for estimations by the eye or very delicate measures of the micrometer. To compare the distances with the apparent diameters the power of the telescope should not be much less than 200, as they will otherwise be too close for the purpose. The instrument ought, moreover, to be as much as possible free from rays that surround a star in common telescopes, and should give the apparent diameters of a double star perfectly round and well-defined, with a deep black division between

between them, as in fig. 6. which represents  $\alpha$  Geminorum as I have often seen it with a power of 460. It will be necessary here to take notice, that the estimations made with one telescope cannot be applied to those made with another: nor can the estimations made with different powers, though with the same telescope, be applied to each other. Whatever may be the cause of the apparent diameters of the stars, they are certainly not of equal magnitude with the same powers in different telescopes, nor of proportional magnitude with different powers in the same telescope. In my instruments I have ever found less diameter in proportion the higher I was able to go in power, and never have I found so small a proportional diameter as when I magnified 6450 times\*; therefore if we would wish to compare any such observations together, with a view to see whether a change in the distance has taken place, it should be done with the very same telescope and power, even with the very same eye-glass or glasses; for others, though of equal power and goodness, would most probably give different proportional diameters of the stars.

In the third class I have placed all those double stars that are more than five but less than  $15''$  asunder; and for that reason, if they should be used for observations on the parallax of the fixed stars, they ought not to be looked upon as quite free from the effects of refraction, &c. In the same manner that the stars in the first and second classes will serve to try the goodness of the most capital instruments, these will afford objects for telescopes of inferior power, such as magnify from 40 to 100 times. The observer may take them in this or the like order:  $\zeta$  Ursa majoris,  $\gamma$  Delphini,  $\gamma$  Arietis,  $\pi$  Bootis,  $\gamma$  Vir-

\* See the measures of the diameter of  $\alpha$  Lyræ. Catalogue of double stars, 5th class.

ginis,

ginis, *Cassiopeæ*, & *Cygni*. And if he can see all these, he may pass over into the second class, and direct his instrument to some of those that were pointed out as objects for the very best telescopes, where, I suppose, he will soon find the want of superior power.

The fourth, fifth, and sixth classes contain double stars that are from 15" to 30", from 30" to 1', and from 1' to 2' or more asunder. Though these will hardly be of any service for the purpose of parallax, I thought it not amiss to give an account of such as I have observed; they may, perhaps, answer another very important end, which also requires a great deal of accuracy, though not quite so much as the investigation of the parallax of the fixed stars. I will just mention it, though foreign to my present purpose. Several stars of the first magnitude have already been observed, and others suspected, to have a proper motion of their own: hence we may surmise, that our sun, with all its planets and comets, may also have a motion towards some particular part of the heavens, on account of a greater quantity of matter collected in a number of stars and their surrounding planets there situated, which may perhaps occasion a gravitation of our whole solar system towards it. If this surmise should have any foundation, it will shew itself in a series of some years; as from that motion will arise another kind of hitherto unknown parallax\*, the investigation of which may account for some part of the motions already observed in some of the principal stars; and for the purpose of determining the direction and quantity of such a motion, accurate observations of the distance of stars that are near enough to be measured with a micrometer, and a very high power of

\* See the note in the rev. Mr. MITCHELL's paper on the Parallax of the Fixed Stars, *Phil. Trans.* vol. LVII. p. 252.

telescopes,

telescopes may be of considerable use, as they will undoubtedly give us the relative places of those stars to a much greater degree of accuracy than they can be had by transit instruments or sectors, and thereby much sooner enable us to discover any apparent change in their situation occasioned by this new kind of systematical parallax, if I may be allowed to use that expression, for signifying the change arising from the motion of the whole solar system.

I shall now endeavour to deliver a theory of the annual parallax of double stars, with the method of computing from thence what is generally called the parallax of the fixed stars, or of single stars of the first magnitude, such as are nearest to us. It may be observed, that the principles upon which I have founded the following theory are of such a nature, that they cannot be strictly demonstrated, in consequence of which they are only proposed as postulata, which have so great a probability in their favour, that they will hardly be objected to by those who are in the least acquainted with the doctrine of chances.

## GENERAL POSTULATA.

1. Let the stars be supposed, one with another, to be about the size of the sun\*.
2. Let the difference of their apparent magnitudes be owing to their different distances, so that a star of the second, third,

\* See Mr. MICHELL's Inquiry into the probable Parallax and Magnitude of the Fixed Stars, Phil. Transf. vol. LVII. p. 234. 236. 237. 240. and Dr. HALLEY on the Number, Order, and Light, of the Fixed Stars, Phil. Transf. vol. XXXI.

or fourth magnitude is two, three, or four times as far off as one of the first\*.

In fig. 7. let OE be the whole diameter of the earth's annual orbit; and let  $a, b, c$ , be three stars situated in the ecliptic, in such a manner that they may be seen all in one line Oabc, when the earth is at O. Let the line Oabc be perpendicular to OE, and draw PE parallel to cO. Then, if Oa, ab, bc, are equal to each other,  $a$  will be a star of the first magnitude,  $b$  of the second, and  $c$  of the third. Let us now suppose the angle OaE, or parallax of the whole orbit of the earth, to be  $1''$  of a degree: then we have  $PEa = OaE = 1''$ : and, because very small angles, having the same subtense OE, may be taken to be in the inverse ratio of the lines Oa, Ob, Oc, &c. we shall have  $ObE = \frac{1}{2}''$ ,  $OcE = \frac{1}{3}''$ , &c. †. Now, when the earth is removed

\* The apparent magnitude is here taken in a stricter sense than is generally used; and by it is rather meant the order into which the stars *ought to be* distinguished than that into which they *are* commonly divided: for as the order of the magnitudes is here to denote the different relative distances, we are to examine carefully the degree of light each star is accurately found to have: and considering then that light diminishes in the inverse ratio of the squares of the distances, we ought to class the stars accordingly. An allowance ought also perhaps to be made, for some loss that may happen to the light of very remote stars in its passage through immense tracts of space, most probably not quite destitute of some very subtle medium. This conjecture is suggested to us by the colour of the very small telescopic stars, for I have generally found them red, or inclining to red; which seems to indicate, that the more feeble and refrangible rays of the other colours are either stopped by the way, or at least diverted from their course by accidental deflections.

† This proves what I have before remarked on the parallax of  $\gamma$  Draconis; for that star, (admitting it to be a star of between the second and third magnitude; which ought to be ascertained by experiments, as mentioned in the note above) by the postulata, will have its place assigned somewhere between  $b$  and  $c$ , and therefore its parallax will be between  $\frac{1}{2}$  and  $\frac{1}{3}$  of the parallax of a star of the first magnitude. And if Dr. BRADLEY thought that he should have perceived a

removed to E, we shall have  $PEb = EbO = \frac{1}{2}''$ , and  $PEa - PEb = aEb = \frac{1}{2}''$ ; that is, the stars  $a, b$ , will appear to be  $\frac{1}{2}''$  distant. We also have  $PEc = EcO = \frac{1}{3}''$ , and  $PEa - PEc = aEc = \frac{2}{3}''$ ; that is, the stars  $a, c$ , will appear to be  $\frac{2}{3}''$  distant, when the earth is at E. Now, since we have  $bEP = \frac{1}{2}''$ , and  $cEP = \frac{1}{3}''$ , therefore  $bEP - cEP = bEc = \frac{1}{2}'' - \frac{1}{3}'' = \frac{1}{6}''$ ; that is, the stars  $b, c$ , will appear to be only  $\frac{1}{6}''$  removed from each other, when the earth is at E.

From what has been said, we may gather the following general expression, to denote the parallax that will become visible in the change of distance between the two stars, by the removal of the earth from one extreme of its orbit to the other. Let  $P$  express the total parallax of a fixed star of the first magnitude,  $M$  the magnitude of the largest of the two stars,  $m$  the magnitude of the smallest\*, and  $p$  the partial parallax to be observed by the change in the distance of a double star; then will  $p = \frac{m-M}{Mm} P$ ; and  $p$  being found by observation will give us  $P = \frac{pMm}{m-M}$ . An example or two will explain this sufficiently. Suppose a star of the first magnitude should have a small star of the twelfth magnitude near it; then will the partial parallax

parallax in  $\gamma$  Draconis, if at most it had amounted to  $2''$ , it follows, that the angle  $OaE$  may nearly amount to  $4$  or  $5''$  for any thing we can conclude to the contrary from those observations.

\* As  $M$  and  $m$  are here taken to express the relative distances of the stars, in measures whereof the distance of the nearest star is taken as unity, those who think the postulata on which these estimations are built cannot be granted, may still use the following formulæ, if instead of the magnitudes  $M, m$ , they put their own estimations of the relative distances of the stars, according to any other method whatever they may think it most eligible to adopt; for the apparent magnitude of stars is here only proposed as the most probable means we have of forming any conjectures about their relative distances.

we are to expect to see be  $\frac{12 \times 1}{12 - 1} P$ ; or  $\frac{1}{11}$ ths of the total parallax of a fixed star of the first magnitude; and if we should, by observation, find the partial parallax between two such stars to amount to  $1''$ , we shall have the total parallax  $P = \frac{1 \times 1 \times 12}{12 - 1} = 1'',0909$ . If the stars are of the third and twenty-fourth magnitude, the partial parallax will be  $\frac{24 - 3}{3 \times 24} = \frac{21}{72} P$ ; and if, by observation,  $p$  is found to be a tenth of a second, the whole parallax will come out  $\frac{1 \times 3 \times 24}{24 - 3} = 0'',3428$ .

It will be necessary to examine some different situations. Suppose the stars, being still in the ecliptic, to appear in one line, when the earth is in any other part of its orbit between O and E; then will the parallax still be expressed by the same algebraic form, and one of the maxima will still lie at O, the other at E; but the whole effect will be divided into two parts, which will be in proportion to each other as radius - sine to radius + sine of the stars distance from the nearest conjunction or opposition.

When the stars are any where out of the ecliptic situated so as to appear in one line *Oabc* at rectangles to OE, the maximum of parallax will still be expressed by  $\frac{m - M}{Mm} P$ ; but there will arise another additional parallax in the conjunction and opposition, which will be to that which is found  $90^\circ$  before or after the sun, as the sine (S) of the latitude of the stars seen at O is to radius (R); and the effect of this parallax will be divided into two parts; half of it lying on one side of the large star, the other half on the other side of it. This latter parallax, moreover, will be compounded with the former, so that

P 2

the



the distance of the stars in the conjunction and opposition will then be represented by the diagonal of a parallelogram, whereof the two semi-parallaxes are the sides; a general expression for

which will be  $\sqrt{\frac{m-M}{2Mm} P^2 \times \frac{SS}{RR} + 1}$ : for the stars will apparently describe two ellipses in the heavens, whose transverse axes will be to each other in the ratio of  $M$  to  $m$  (fig. 8.), and  $Aa$ ,  $Bb$ ,  $Cc$ ,  $Dd$ , will be cotemporary situations. Now, if  $bQ$  be drawn parallel to  $AC$ , and the parallelogram  $bqBQ$  completed, we shall have  $bQ = \frac{1}{2}CA - \frac{1}{2}ca = \frac{1}{2}Cc = \frac{1}{2}p$ , or semi-parallax  $90^\circ$  before or after the sun, and  $Bb$  may be resolved into, or is compounded of,  $bQ$  and  $bq$ ; but  $bq = \frac{1}{2}BD - \frac{1}{2}bd =$  the semi-parallax in the conjunction or opposition. We also have  $R : S :: bQ : bq = \frac{p^S}{2R}$ ; therefore the distance  $Bb$  (or  $Dd$ ) =

$\sqrt{\left[\frac{p}{2}\right]^2 + \left[\frac{p^S}{2R}\right]^2}$ ; and by substituting the value of  $p$  into this ex-

pression we obtain  $\sqrt{\frac{m-M}{2Mm} P^2 \times \frac{SS}{RR} + 1}$ , as above. When the stars are in the pole of the ecliptic,  $bq$  will become equal to  $bQ$ , and  $Bb$  will be  $.7071 P \frac{m-M}{Mm}$ .

Hitherto we have supposed the stars to be all in one line  $Oabc$ ; let them now be at some distance, suppose  $5''$  from each other, and let them first be both in the ecliptic. This case is resolvable into the first; for imagine the star  $a$ , fig. 9. to stand at  $x$ , and in that situation the stars  $x$ ,  $b$ ,  $c$ , will be in one line, and their parallax expressed by  $\frac{m-M}{Mm} P$ . But the angle  $aEx$  may be taken to be equal to  $aOx$ ; and as the foregoing form gives us the angles  $xEb$ ,  $xEc$ , we are to add  $aEx$ , or  $5''$  to  $xEb$ , and we shall have  $aEb$ . In general, let the distance

tance of the stars be  $d$ , and let the observed distance at E be  $D$ ; then will  $D = d + p$ , and therefore the whole parallax of the annual orbit will be expressed by  $\frac{DMm - dMm}{m - M} = P$ .

Suppose the two stars now to differ only in latitude, one being in the ecliptic, the other, for instance,  $5''$  north, when seen at O. This case may also be resolved by the former; for imagine the stars  $b$ ,  $c$ , fig. 7. to be elevated at rectangles above the plane of the figure, so that  $aOb$ , or  $aOc$ , may make an angle of  $5''$  at O: then, instead of the lines  $Oabc$ ,  $Ea$ ,  $Eb$ ,  $Ec$ ,  $EP$ , imagine them all to be planes at rectangles to the figure; and it will appear, that the parallax of the stars in longitude must be the same as if the small star had been without latitude. And since the stars  $b$ ,  $c$ , by the motion of the earth from O to E, will not change their latitude, we shall have the following construction for finding the distance of the stars  $ab$ ,  $ac$ , at E, and from thence the parallax P. Let the triangle  $ab\beta$ , fig. 10. represent the situation of the stars;  $ab$  is the subtense of  $5''$ , that being the angle under which they are supposed to be seen at O. The quantity  $b\beta$  by the former theorem is found  $\frac{m - M}{Mm} P$ , which is the partial parallax that would have been seen by the earth's moving from O to E, had both stars been in the ecliptic; but on account of the difference in latitude it will now be represented by  $a\beta$ , the hypotenuse of the triangle  $ab\beta$ : therefore, in general, putting  $ab = d$ , and  $a\beta = D$ , we have  $\frac{\sqrt{DD - dd} \times Mm}{m - M} = P$ . Hence D being taken by observation and  $d$ , M, and  $m$ , given, we obtain the total parallax.

If the situation of the stars differs in longitude as well as latitude, we may resolve this case by the following method.

Let

Let the triangle  $ab\beta$ , fig. 11. represent the situation of the stars,  $ab = d$  being their distance seen at O,  $a\beta = D$  their distance seen at E. That the change  $b\beta$  which is produced by the earth's motion will be truly expressed by  $\frac{m-M}{Mm} P$ , may be proved as before, by supposing the star  $a$  to have been placed at  $\alpha$ . Now let the angle of position  $ba\alpha$  be taken by a micrometer\*, or by any other method that may be thought sufficiently exact; then, by solving the triangle  $ab\alpha$ , we shall have the longitudinal and latitudinal differences  $a\alpha$  and  $b\alpha$  of the two stars. Put  $a\alpha = x$ ,  $b\alpha = y$ , and it will be  $x + b\beta = aq$ , whence  $D = \sqrt{x + \frac{m-M}{Mm} P} + y$ ; and  $\frac{\sqrt{D^2 - y^2} \times M^2 m - x M m}{m - M} = P$ .

If neither of the stars should be in the ecliptic, nor have the same longitude or latitude, the last theorem will still serve to calculate the total parallax whose maximum will lie in E. There will, moreover, arise another parallax, whose maximum will be in the conjunction and opposition, which will be divided, and lie on different sides of the large star; but as we know the whole parallax to be exceedingly small, it will not be necessary to investigate every particular case of this kind; for, by reason of the division of the parallax, which renders observations taken at any other time, except where it is greatest, very unfavourable, the forms would be of little use.

To finish this theory, I shall only add a general observation on the time and place where the maxima of parallax will happen.

\* The position of a line passing through the two stars, with the parallel of declination of the largest of them, may be had by the micrometer I invented for this purpose in the year 1779, of which a description has been given in a former paper; whence, by spherical trigonometry, we easily deduce their position  $ba\alpha$  fig. 11. with regard to the ecliptic.

When

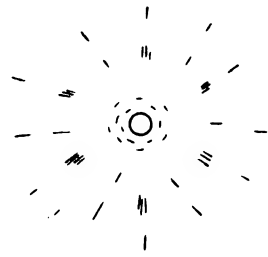
*Fig. 1.*



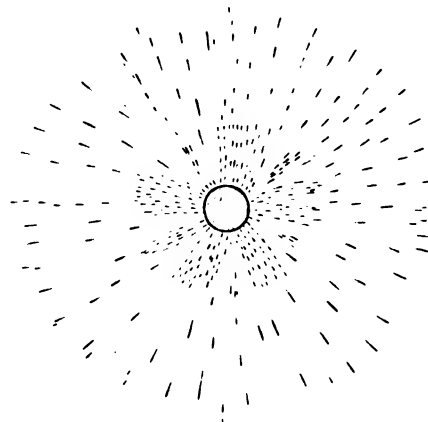
*Fig. 2.*



*Fig. 3.*



*Fig. 5.*





When two unequal stars are both in the ecliptic, or, not being in the ecliptic, have equal latitudes, north or south, and the largest star has most longitude, the maximum of the apparent distance will be when the sun's longitude is  $90^\circ$  more than the stars, or when observed in the morning; and the minimum when the longitude of the sun is  $90^\circ$  less than that of the star, or when observed in the evening.

When the small star has most longitude, the maximum and minimum, as well as the time of observation, will be the reverse of the former.

When the stars differ in latitudes, this makes no alteration in the place of the maximum or minimum, nor in the time of observation; that is to say, it is immaterial whether the largest star has the least or the most latitude of the two stars.

XII. *Catalogue of Double Stars.* By Mr. Herschel, F. R. S.  
communicated by Dr. Watson, Jun.

Read January 10, 1782.

INTRODUCTORY REMARKS.

THE following catalogue contains not only double-stars, but also those that are treble, double-double, quadruple, double-treble, and multiple. The particulars I have given of them are comprehended under the following general heads.

I. The names of the stars and number in FLAMSTEAD'S Catalogue; or, if not contained therein, such a description of their situation as will be found sufficient to point them out.

II. The comparative size of the stars. On this occasion I have used the terms equal, a little unequal, pretty unequal, considerably unequal, very unequal, extremely unequal, and excessively unequal, as expressing the different gradations to which I have endeavoured to affix always the same meaning.

III. The colours of the stars as they appeared to me when I viewed them. Here I must remark, that different eyes may perhaps differ a little in their estimations. I have, for instance, found, that the little star which is near  $\alpha$  Herculis, by some to whom I have shewn it has been called green, and by others blue. Nor will this appear extraordinary when we recollect that there are blues and greens which are very often, particularly by candle-light, mistaken for each other. The situation will also affect the

the colour a little, making a white star appear pale red when the altitude is not sufficient to clear it of the vapours. It is difficult to find a criterion of the colours of stars, though I might in general observe that Aldebaran appears red, *Lyra* white, and so on; but when I call the stars garnet, red, pale red, pale rose-colour, white inclining to red, white, white inclining to blue, blueish white, blue, greenish, green, dusky, I wish rather to refer to the double stars themselves to explain what is meant by those terms.

IV. The distances of the stars are given several different ways. Those that are estimated by the diameter can hardly be liable to an error of so much as one quarter of a second; but here must be remembered what I have before remarked on the comparative appearance of the diameters of stars in different instruments. Those that are measured by the micrometer, I fear, may be liable to an error of almost a whole second; and if not measured with the utmost care, to near 2". This is, however, to be understood only of single measures; for the distance of many of them that have been measured very often in the course of two years observations can hardly differ so much as half a second from truth, when a proper mean of all the measures is taken. As I always make the wires of my micrometer outward tangents to the apparent diameter of the stars, all the measures must be understood to include both their diameters; so that we are to deduct the two semi-diameters of the stars if we would have the distance of their centers. What I have said concerns only the wire micrometers, for my last new micrometer is of a such a construction, that it immediately gives the distance of the centers and its measures (as far as in a few months I have been able to find out) may be relied on to about one-tenth of a second, when a mean of three observations is taken. When I have



added *inaccurate*, we may suspect an error of 3 or 4". *Exactly estimated* may be taken to be true to about one-eighth part of the whole distance; but only *estimated*, or *about*, &c. is in some respect quite undetermined; for it is hardly to be conceived how little we are able to judge of distances when, by constantly changing the powers of the instrument, we are as it were left without any guide at all. I should not forget to add, that the measure of stars, whereof one is extremely small, must claim a greater indulgence than the rest on account of the difficulty of seeing the wires when the field of view cannot be sufficiently enlightened.

V. The angle of position of the stars I have only given with regard to the parallel of declination, to be reduced to that with the ecliptic as occasion may require. The measures always suppose the large star to be the standard, and the situation of the small one is described accordingly. Thus in figure 12. AB represents the apparent diurnal motion of a star in the direction of the parallel of declination AB; and the small star is said to be south preceding at *mn*, north preceding at *op*, south following at *qr*, and north following at *st*. The measure of these angles, I believe, may be relied upon to 2° or at most 3°, except when mentioned inaccurate, where an error amounting to 5° may possibly take place. In mere estimations of the angle, without any wires at all, an error may amount to at least 10°, when the stars are near each other.

VI. The dates when I first perceived the stars to be double, treble, &c. are marked in the margin of each star.

To shorten the work as much as possible, I have put L. for the large star; S. for the small star; w. for white; r. for red; d. for dusky; n. for north; s. for south; and have likewise

occa-

occasionally used other abbreviations that will be easily understood.

It may be seen, that this catalogue is yet in a very imperfect state, many of the stars not having even the principal elements of distance and position determined with any degree of accuracy; but having already mentioned the reason why I give it imperfect as it is, I can only add that my endeavours will not be wanting soon to remove those defects. However, since this can only be a work of some time, we may hope, in the mean while, that many lovers of the science will turn their thoughts upon the same subject.

## CATALOGUE OF DOUBLE STARS.

### FIRST CLASS.

1.  $\epsilon$  Bootis. FLAMST. 36. Ad dextrum femur in perizomate.  
 Sept. 9. Double. Very unequal. L. reddish; S. blue, or  
 1779. rather a faint lilac. A very beautiful object. The  
 vacancy, or black division between them, with 227  
 is  $\frac{1}{2}$  diameter of S.; with 460,  $1\frac{1}{2}$  diameter of L.;  
 with 932, near 2 diameters of L.; with 1159, still  
 farther; with 2010 (extremely distinct)  $2\frac{1}{2}$  diameters  
 of L. These quantities are a mean of two years ob-  
 servation. Position  $31^{\circ} 34'$  n. preceding.
2.  $\xi$  Ursa majoris. FL. 53. In dextro posteriore pede.  
 May 2, Double. A little unequal. Both w. and very  
 1780. bright. The interval with 222 is  $\frac{2}{3}$  diameter of L.;  
 with 227, 1 diameter of L.; with 278, near  $1\frac{1}{2}$  dia-  
 meter of L. Position  $53^{\circ} 47'$  f. following.

Q 2

3.  $\sigma$  Coronæ

3.  $\sigma$  Coronæ borealis, FL. 17.

Aug. 7, Treble. The two nearest pretty unequal; the  
 1780. third very faint with powers lower than 460. The  
 two nearest both w.; the third d. Interval of the  
 two nearest with 227, full  $1\frac{1}{2}$  diameter of L.; with  
 460, 2 diameters of L. Position  $77^{\circ} 32'$  n. pre-  
 ceding. Distance of the third from L.  $24''$  by exact  
 estimation. Position  $25^{\circ}$  n. following by estimation.

## 4. In constellatione Draconis, FL. 16.

Aug. 8, Double. It is the star to which a line drawn from  
 1780.  $\nu$  through  $\mu$  points, at nearly the same distance from  
 $\mu$  as  $\mu$  from  $\nu$ . Considerably unequal. L. w.; S. w.  
 inclining to r. With 222, 1 diameter of L.; with  
 278,  $1\frac{1}{2}$  diameter of L. Position  $24^{\circ} 0'$  f. following.  
 There is a third star, at some distance, preceding.

5.  $\sigma$  Cassiopeæ, FL. 8. In dextro cubito,

Aug. 31, Double. It is the star at the vertex of a telescopic  
 1780. isosceles triangle turned to the south. Very unequal.  
 L. w. a little inclining to r.; S. d. With 222, near  
 1 diameter of L.; with 460,  $1\frac{1}{2}$  diameter of L.  
 Position  $60^{\circ} 28'$  n. preceding.

## 6. Quæ infra oculum Lyncis, FL. 12.

Oct. 3, A curious treble star. Two nearest pretty unequal.  
 1780. L. w.; S. w. inclining to rose colour. With 227,  
 about  $\frac{1}{2}$  diameter; with 460, full  $\frac{1}{2}$  diameter of S.  
 Position  $88^{\circ} 37'$  f. preceding. The first and third  
 considerably unequal; second and third pretty un-  
 equal. The third pale r. Distance from the first  
 $9'' 23'''$ ; too difficult to be extremely exact. Position  
 with regard to the first  $32^{\circ} 33'$  n. preceding.

7.  $\delta$  Draconis,

7. *β* Draconis, FL. 39. Trium in recta, in prima inflectione colli, borea.

Oct. 3. A minute double star. Extremely unequal, the  
1780. small star being a fine lucid point. L. w.; S. inclining to r. With 227,  $\frac{1}{2}$  diameter of L.; with 460, full  $1\frac{1}{2}$  diameter of L.; with 932 (extremely fine) full 2 diameters of L. Position  $77^{\circ} 8'$  n. following. A third star at some distance; dusky r. Position  $63^{\circ} 22'$  n. following.

8.  $\epsilon$  Draconis, FL. 63. In quadrilatero inflexionis primæ.

Oct. 3. A very minute double star. Excessively unequal;  
1780. the small star can only be seen when the air is perfectly clear. L. w.; S. d. With 227, less than 1 diameter of L.; with 278, not a diameter of L. Position  $63^{\circ} 14'$  n. preceding. A pretty large third star at about 3 or 4'. Position of this third star with  $\epsilon$   $88^{\circ} 16'$  n. following.

9. In cauda Lyncis media, FL. 38.

Nov. 24. Double. Very unequal. L. w.; S. inclining to  
1780. r. With 227, extremely close; with 460, at least  $\frac{1}{2}$  diameter of S. A very fine object. Position  $25^{\circ} 51'$  f. preceding. A proper motion is suspected in one of the stars.

10. In sinistro anteriore pede Monocerotis, FL. 11.

Feb. 15. A curious treble star; may appear double at first  
1781. sight; but with some attention we see that one of them again is double. The first, or single star, is the largest; the other two are both smaller, and almost equal, but the preceding of them is rather larger than the following. They are all w. The two nearest with 227, 1 diameter of the preceding, or nearly

nearly  $1\frac{1}{2}$  of the following ; with 460,  $1\frac{1}{2}$  diameter of the preceding. Position of the two nearest  $11^{\circ} 32'$  f. following. For an account of the single star, see the second class. As perfect as I have seen this treble star with 460, it is one of the most beautiful sights in the heavens ; but requires a very fine evening.

11. In constellatione Cancrī, FL. 11.

Mar. 13, Double. Considerably unequal. Both pale r.  
1781. With 227, 1 full diameter of L. ; with 460, about  $1\frac{1}{2}$  diameter of L. Position  $85^{\circ} 10'$  n. preceding.

12. *d* Serpentis, FL. 59. In Cauda.

July 17, Double. Very unequal. L. reddish w. ; S. fine  
1781. blue. With 227, 1 full diameter of L. ; with 278,  $1\frac{1}{2}$  diameter of L. Position  $44^{\circ} 33'$  n. preceding.

13. In constellatione Aquilæ, near FL. 37.

July 25, A curious treble star. It is the last star of a tele-  
1781. scopic trifolium n. following *k*, similar to that in the hand of Aquarius. The two nearest very unequal ; the third star excessively small, and not visible with 227. The two nearest with 460, no more than  $\frac{1}{2}$  diameter of L. ; the farthest about 7 or 8".

14. In constellatione Aquilæ, FL. 24.

July 30, Double. In HARRIS's maps it is the star in the  
1781. elbow of Antinous. Excessively unequal ; the small star is but just visible with 227 ; but with 460 it is pretty strong. L. pale r. ; S. d. With 227, 1 full diameter of L. ; with 460,  $1\frac{1}{2}$  diameter of L. Position  $72^{\circ} 0'$  f. following.

15. *i* Bootis, FL. 44.

Aug. 17, Double. In HARRIS's maps it is marked *i*, but has  
1781. no letter in FL. Atlas. Considerably unequal. Both

w.

w. With 227 they seem almost to touch, or at most  $\frac{1}{2}$  diameter of S. asunder; with 460,  $\frac{1}{2}$  or  $\frac{3}{4}$  diameter of S. This is a fine object to try a telescope, and a miniature of  $\alpha$  Geminorum. Position  $29^{\circ} 54'$  n. following.

16.  $\eta$  Coronæ borealis, FL. 2.

Sept. 9. Double. A little unequal. They are whitish stars.

1781. They seem in contact with 227, and though I can see them with this power, I should certainly not have discovered them with it; with 460, less than  $\frac{1}{2}$  diameter; with 932, fairly separated, and the interval a little larger than with 460. I saw them also with 2010, but they are so close that this power is too much for them, at least when the altitude of the stars is not very considerable; with 460 they are as fine a miniature of  $i$  Bootis as that is of  $\alpha$  Geminorum. Position  $59^{\circ} 19'$  n. following.

17. In constellatione Bootis, near FL 51.

Sept. 10. Double. It is a star near  $\mu$  not marked in FLAM-

1781. STEAD's Catalogue. Considerably unequal. Both dusky w. inclined to r. The interval with 460 is  $\frac{1}{2}$  diameter of S. The position of the small star is turned towards  $\mu$  a little following the line which joins L to  $\mu$  Bootis. See  $\mu$  Bootis in the sixth class.

18. In constellatione Coronæ borealis.

Sept. 10. Double. It is the smallest of two telescopic stars

1781. between  $\theta$  and  $\delta$ , not contained in FL. Cat. Equal. Both d. With 460, about  $1\frac{1}{2}$  diameters. Position  $21^{\circ} 0'$  n. following.

19. *b* Draconis, near FL. 19.

Sept. 10. One of the most minute of all the double stars I  
 1781. have hitherto found. It is the small telescopic star near the preceding *b* Draconis. Considerably unequal. Both dusky w. inclining to r. With 460, they seem in contact; I have however had a very good view of a small dark division between them. Position (by exact estimation)  $25$  or  $30^\circ$  f. preceding. They are too minute for any micrometer I have. It is in vain to look for them if every circumstance is not favourable. The observer as well as the instrument must have been long enough out in the open air to acquire the same temperature. In very cold weather, an hour at least will be required; but in a moderate temperature, half an hour will be sufficient.

## 20. In dextro humero Orionis, FL. 52.

OA. 1. Double. A little unequal. Both w. a little in-  
 1781. clining to pale r. With 227,  $\frac{1}{2}$  diameter; with 460,  $\frac{1}{2}$  diameter. Position  $69^\circ 41'$  f. preceding.

21. *c* Trianguli, near FL. 12. and 13.

OA. 8. Double. It is the most north of a small telescopic  
 1781. trapezium of unequal stars. Extremely unequal. With 460,  $\frac{1}{2}$  diameter of L. Position (by estimation)  $55$  or  $60^\circ$  n. preceding.

22.  $\pi$  Orionis, FL. 33. Duarum præcedentium  $13^{\text{am}}$  ( $\omega$ ) antecedens.

OA. 22. Double. Considerably unequal. L. w.; S. w.;  
 1781. inclining to blue. With 227, they seem almost in contact; with 460,  $\frac{1}{2}$  diameter of S. Position  $60^\circ 55'$  n. following. A very pleasing object and easily seen.

23. In posterioribus femoribus Canis minoris.

Nov. 21, 1781. A most minute double star. It is the small telescopic star following Procyon. A little unequal. Both w. With 278,  $\frac{1}{2}$  of a diameter of S.; with 460, near  $\frac{1}{4}$  of a diameter of S. They are closer than  $\gamma$  Coronæ, because their diameters, by which they are estimated, are smaller. Position  $27^{\circ} 12'$  f. following. To see this very minute double star well, Procyon should be near its meridian altitude. There is a small telescopic star preceding the double star. Distance  $1^{\circ} 59'' 39'''$  from center to center.

24.  $\zeta$  Cancri. No. 16.

Nov. 21, 1781. A most minute treble star. It will at first sight appear as only a double star, but with proper attention, and under favourable circumstances, the preceding of them will be found to consist of two stars, which are considerably unequal. The largest of these is larger than the single star; and the least of the two is less than the single star. The first and second (in the order of magnitude) pretty unequal. The second and third pretty unequal. The two nearest both pale r. or r. With 278, but just separated; with 460,  $\frac{1}{4}$  diameter of S. Position  $86^{\circ} 32'$  n. following. For measures relating to the third or single star see  $\zeta$  Cancri in the third class of double stars.



## SECOND CLASS OF DOUBLE STARS.

1.  $\dagger \alpha$  Geminorum, FL. 66. In capite præcedentis II.

April 8, Double. A little unequal. Both w. The vacancy  
 1778. between the two stars, with a power of 146, is 1  
 diameter of S.; with 222, a little more than 1 dia-  
 meter of L.; with 227,  $1\frac{1}{2}$  diameter of S.; with  
 460, near 2 diameters of L.; (see fig. 6.) with 754, 2  
 diameters of L.; with 932, full 2 diameters of L.;  
 with 1536 (very fine and distinct) 3 diameters of L.;  
 with 3168, the interval extremely large, and still  
 pretty distinct. Distance by the micrometer  $5''$ , 156.  
 Position  $32^{\circ} 47'$  n. preceding. These are all a mean  
 of the last two years observations, except the first  
 with 146.

2.  $\dagger \alpha$  Herculis, FL. 64. In capite.

Aug. 29, A beautiful double star. Very unequal. L. n.;  
 1779. S. blue inclining to green; the colours with every  
 power the same. The interval with 222,  $1\frac{1}{2}$  dia-  
 meter of L.; with 227, above 2 diameters of L.; with  
 932, above 3 diameters of L. Distance  $4''$ , 966. All  
 a mean of two years observations. A single measure  
 with my last new micrometer, from center to center,  
 $4'' 34'''$ . Position  $30^{\circ} 35'$  f. following.

3.  $* \rho$  Herculis, FL. 75. Trium in sinistro femore, tertia.

Aug. 29, Double. Pretty unequal. Both w. With 227,  
 1779.  $1\frac{1}{2}$  diameter of L.; with 460, 2 diameters of S.  
 Distance  $2''$ , 969. Position  $30^{\circ} 21'$  n. preceding.  
 The measures a mean of two years observations.

4. \*  $\rho$  Serpentarii, FL. 79. Tres has sequitur, quasi supra mediam.

Aug. 29. Double. Considerably unequal. L. w.; S. inclining to r. With 227,  $1\frac{1}{2}$  diameter of L.; with 1779, much above 2 diameters of L. Position  $9^{\circ} 14'$  f. following. Mean of two years observations.

5. et 6. \*  $\epsilon$  Lyra, FL. 4. and 5.

Aug. 29. A very curious double-double star. At first sight it appears double at some considerable distance, and by attending a little we see that each of the stars is a very delicate double star. The first set consists of stars that are considerably unequal. The stars of the second set are equal, or the preceding of them rather larger than the following. The colour of the stars in the first set L. very w.; S. a little inclining to r. In the second set both w. The interval between the stars of the unequal set, with a power of 227, is full 1 diameter of L.; with 460, near  $1\frac{1}{2}$  diameter of L.; with 932, full  $1\frac{1}{2}$  diameter; with 2010,  $2\frac{1}{2}$  diameters. The interval between the equal set with a power of 227 is almost  $1\frac{1}{2}$  diameter of either; with 460, full  $1\frac{1}{2}$  diameter; with 932, 2 diameters; with 2010,  $2\frac{1}{2}$  diameters. These estimations are a mean of two years observations. Position of the unequal set  $56^{\circ} 0' n.$  following. Position of the equal set  $72^{\circ} 57' f.$  following.

7. \*  $\zeta$  Aquarii, FL. 55. Trium in manu dextra precedens.

Sept. 12. Double. Equal, or the preceding rather the largest. Both w. With 227,  $1\frac{1}{2}$  diameter; with 1779,  $1\frac{1}{2}$  diameter; with 460, 2 diameters; with 932, near 2 diameters; with 2010,  $2\frac{1}{2}$  diameters;

R 2

with

with 2010, pretty distinct; but too tremulous to estimate. With my 20 feet reflector, power 600, full 2 diameters, very distinct. Position,  $71^{\circ} 39'$  n. following. Distance  $4''.56$ , mean of two years observation.

8.  $\zeta$  Coronæ borealis, FL. 7.

Oa. 1. Double. Considerably unequal. L. fine w. S. w. inclining to r. With 222, almost 3 diameters of L. Distance  $5''.468$ . Position  $25^{\circ} 51'$  n. preceding, mean of two years observations.

9.  $\lambda$  Orionis, FL. 39. In capite nebulosa.

Oa. 7. Quadruple, or rather a double star and two more at a small distance. The double star considerably unequal. L. w.; S. pale rose colour. With 222,  $1\frac{1}{2}$  diameter of L.; with 449, above two diameters of L. Distance  $5''.833$ , a mean of all the measures. Position  $45^{\circ} 14'$  n. following. As every one of the four stars is perfectly distinct, it is evident, the whole appeared nebulous to FLAMSTEAD for no other reason than because his telescope had not sufficient power to distinguish them.

10. and 11.  $\sigma$  Orionis, FL. 48. Ultimam cinguli præcedit ad austrum.

Oa. 7. A double-treble star, or two sets of treble stars, almost similarly situated. Preceding set. The two nearest equal; the third larger and, compared with either of the former two, pretty unequal. The two nearest with 222, about 2 diameters. Position of the following star of the two nearest with the third  $66^{\circ} 35'$  f. preceding. Position of the two nearest, by exact estimation, 2 or  $3^{\circ}$  n. following or f. preceding

ceding the following set. The two nearest very unequal. The largest of the two and the farthest considerably unequal. L. w.; S. blueish. The two nearest with 222, about  $2\frac{1}{2}$  diameters of L.; the two farthest  $43'' 12'''$ . Position of the two nearest  $5^{\circ} 5' n.$  following. Position of the two farthest  $29^{\circ} 4' n.$  following. A pretty object with 227.

12.  $\alpha$  Pictoris, FL. ultima. In nodo duorum linorum.

OA. 19. Double. Considerably unequal. Both w. With  
1779. 222, not quite 2 diameters of L.; with 460, about 3 diameters of L. Distance  $5''$ , 123 mean measure. Position  $67^{\circ} 23' n.$  preceding.

13.  $\mu$  Draconis, FL. 21. In lingua,

OA. 19. Double. Equal. Both w. With 227,  $1\frac{1}{2}$  dia-  
1779. meter; with 460,  $2\frac{1}{2}$  diameters. Distance  $4''$ , 354 mean measure. Position  $37^{\circ} 38' s.$  preceding or n. following.

14.  $\alpha$  Aurigæ, FL. 4.

OA. 30. Double. Very unequal. L. w.; S. r. With  
1779. 227, almost 2 diameters of L.; with 460, full 3 diameters of L. Position  $82^{\circ} 37' n.$  preceding.

15.  $\psi$  Cygni, FL. 24. In ala dextra.

Nov. 2. Double. Extremely unequal; the small star a mere  
1779. point. L. w.; S. r. With 227, near  $1\frac{1}{2}$  diameter of L.; with 278, near  $1\frac{1}{2}$  diameter of L.; with 460, 2 diameters of L. Position  $89^{\circ} 32' n.$  preceding.

16.  $\xi$  Cephei, FL. 17. In pectore.

Nov. 7. A fine double star. Considerably unequal. L. w.  
1779. inclining to r.; S. dusky grey. With 222, nearly 2 diameters of L. Single measure  $5'', 00$ . Position  $20^{\circ} 18' n.$  preceding.

17. \* In finistro anteriore pede Monocerotis, FL. 11;  
 Dec. 5, Double. With 222, about  $1\frac{1}{2}$  diameter. Position  
 1779, (taken Oct. 20, 1781.) with the farthest of the other  
 two stars  $31^{\circ} 38'$  f. following. See the tenth star in  
 the first class.
18.  $\xi$  Bootis, FL. 37.  
 April 9, Double. Very unequal. L. pale r. or nearly r.  
 1780. S. garnet, or deeper r. than the other. With 222,  
 $1\frac{1}{2}$  diameter of L., with 460, full 3 diameters of  
 L. Distance  $3'' 23'''$  single measure. Position  
 $65^{\circ} 53'$  n. following.
19.  $\gamma$  Serpentarii, FL. 5.  
 May 2, Double. It is a star in the body of Cancer, and  
 1780. the double star is at the angular point of the three  
 telescopic  $\gamma$ 's making a rectangle. Pretty unequal.  
 Both w. With 227,  $1\frac{1}{2}$  diameter of L. Position  
 $82^{\circ} 10'$  f. preceding.
20. and 21.  $\xi$  Libræ, FL. ultima.  
 May 23, Double double. The first set very unequal. L.  
 1780. fine w. With 227, nearly 2 diameters of L\*. By  
 the micrometer  $6'' 23'''$ , but too large a measure.  
 Position  $1^{\circ} 23''$  n. following. The other set both  
 small and obscure. With 227, perhaps 5 or 6 of  
 their diameters asunder.
22.  $\epsilon$  Persei, FL. 45. In finistro genu.  
 Aug. 2, Double. Extremely unequal. L. w.; S. d. With  
 1780. 222,  $2\frac{1}{2}$  diameters of L. Position  $81^{\circ} 28'$  f. fol-  
 lowing, a little inaccurate. A third star near at  
 about  $1\frac{1}{2}$  or  $1\frac{1}{4}$  min.

\* In a future collection this set will be found as a treble star of the first class, the large white star, with a power of 460 and 932, appearing to be two stars.

23. In

23. In constellatione Serpentarii, near FL. 11.

Aug. 7, Double. It is the smallest and preceding of two in  
1780. the finder. Pretty unequal. L. pale r.; S. dusky r.  
With 222, about  $1\frac{1}{2}$  diameter of L.; with 278,  
about  $1\frac{1}{2}$  diameter of L.; with 460, above 2 dia-  
meters of L. Position  $46^{\circ} 24'$  n. preceding. A  
little inaccurate.

24. In constellatione Aquarii, FL. 108. In sequenti flexu 5<sup>a</sup>  
ad A.

Aug. 23, Double. In HARRIS's maps it is marked *i*. Un-  
1780. equal. With 227, 2 diameters; with 460, about  
3 diameters.

25.  $\delta$  Cygni, FL. 52.

Sept. 8, Double. Extremely unequal. L. w. inclining to  
1780. r.; S. d. and extremely faint; with 227,  $2\frac{1}{2}$  dia-  
meters of L.; with 460, about 4 diameters of L.  
or more. Position  $28^{\circ} 17'$  n. following.

26. In constellatione Orionis, near FL. 42. In longo ensis.

Oct. 23, Double. It is the most north of three telescopic  
1780. stars in a line at the end of a cluster near *c*. Ex-  
tremely unequal. L. w.; S. d. With 278,  $1\frac{1}{2}$   
diameter of L. Position  $26^{\circ} 5'$  n. following.

27.  $\delta$  Geminorum, FL. 55. In inguine sinistro sequentis II.

Mar. 13, Double. Extremely unequal. L. w. inclining to  
1781. r.; S. r. With 227, about  $2\frac{1}{2}$  full diameters of L.;  
with 460, 4 or 5 diameters. Position  $85^{\circ} 51'$  f.  
preceding.

28. In constellatione Aquilæ, near FL. 54.

July 23, Double. It is a star following *o*. Excessively un-  
1781. equal. The small star is not visible with 227, nor  
with 278. It is visible with 460; but not without  
attention.

- attention. Distance with 460, about 4 or 5 diameters of L. Position, by very exact estimation,  $36^{\circ} 28'$  n. preceding.
29. In constellatione Aquilæ, near FL. 63. In medio capite.  
 July 31, Double. It is the star at the vertex of a telescopic  
 1781. isosceles triangle near  $\gamma$ . Extremely unequal. Both  
 r. With 460, 2 diameters of L. Position  $75^{\circ} 48'$   
 n. preceding.
30.  $\zeta$  Sagittæ, FL. 8. Trium in arundine sequens.  
 Aug. 23, Double. Extremely unequal. The small star  
 1781. brighter with 460 than with 227 or with 278; with  
 460, between 4 or 5 diameters of L.; with 278,  $2\frac{1}{2}$   
 diameters of L. Distance  $5'' 27'''$  inaccurate. Posi-  
 tion  $34^{\circ} 10'$  n. preceding.
31. In constellatione Draconis, FL. 56.  
 Sept. 6, Double. A little unequal. Both w. With 460,  
 1781. near 3 diameters. Distance  $5'' 7'''$ .
32. In constellatione Sagittæ, near FL. 4.  
 Sept. 7, Double. It is the star north following  $\epsilon$ . L. pale  
 1781.  $\epsilon$ ; S. d. Distance  $5'' 3'''$  inaccurate.
33.  $\beta$  Orionis, FL. 19. In sinistro pede splendida.  
 Oct. 1, Double. Extremely unequal. L. w.; S. inclin-  
 1781. ing to r. With 227,  $2\frac{1}{2}$  or  $2\frac{1}{4}$  diameters of Rigel.  
 With 460, more than 3 diameters of L. Distance  
 $6'' 27'''$ . Position  $68^{\circ} 12'$  f. preceding. The small  
 star not wanting apparent magnitude is better to be  
 seen with my power of 227 than with 460.
34.  $\gamma$  Trianguli, FL. 6.  
 Oct. 8, Double. It is marked  $b$  in the small triangle of  
 1781. HARRIS's maps. Very unequal. L. pale r. or red-  
 dish w.; S. blueish r. With 227, full  $1\frac{1}{4}$  diameter  
 of

of L. with 460, full  $1\frac{1}{2}$  diameter of L. Position  $4^{\circ} 23'$  n. following. A pretty object, somewhat resembling  $\alpha$  Herculis, but smaller and not so bright.

35. In constellatione Trianguli, near  $\beta$  L. 6.

Oct. 8. Double. It is the star following. Equal. Both  
1781. dusky w. With 460, about  $2\frac{1}{2}$  diameters.

36. In constellatione Eridani,  $\beta$  L. 32.

Oct. 22. Double. Considerably unequal. L. reddish w.;  
1781. S. blue. Distance  $4'' 19'''$ . Position  $73^{\circ} 23'$  n. preceding.

37. In capite Monocerotis.

Oct. 22. Double. It is one of a cluster of six telescopic  
1781. stars, arranged in pairs.

38. In constellatione Bootis.

Dec. 24. Double. It is the most north and largest of three  
1781. in a line, f. following  $\beta$  L. 15. Considerably unequal,  
L. w.; S. inclining to r. Distance  $3'' 10'''$ . Position  $83^{\circ} 5'$  f. preceding.

### THIRD CLASS OF DOUBLE STARS.

1.  $\theta$  Orionis,  $\beta$  L. 41. Trium contiguarum in longo ensis media.

Nov. 11. Quadruple. It is the small telescopic Trapezium  
1776. in the Nebula. Considerably unequal. The most southern star of the following side of the Trapezium is the largest; the star in the opposite corner is the smallest; the remaining two are nearly equal. L. pale r.; the star preceding L. inclined to garnet; following L. inclined to garnet; opposite to L. d. With 460, the stars are all full, round, and well-defined.



The two stars in the preceding side distance  $8''$ ,786; in the southern side,  $12''$ ,812; in the following side  $15''$ ,268; in the northern side,  $20''$ ,396.

2.  $\zeta$  Ursæ majoris, FL. 59. Trium in cauda media.

Aug. 17, Double. Considerably unequal. L. w; S. w;  
1779. inclining to pale rose colour. Distance  $14''$ ,5 by two years observation, not a mean but that which I suppose nearest the truth. Position  $56^\circ 46'$  f. following.

3.  $\eta$  Cassiopeæ, FL. 24. In cingulo.

Aug. 17, Double. Very unequal. L. fine w.; S. fine gar-  
1779. net, both beautiful colours. Distance  $11''$ ,275 mean measure. Position  $27^\circ 56'$  n. following.

4. In extremitate pedis Cassiopeæ, FL. 55. Ptolemæi.

Aug. 17, Double. Extremely unequal. L. w.; S. blueish r.  
1779. Distance  $7''$ ,5 single measure. Position  $10^\circ 37'$  f. following †.

5.  $\ast \gamma$  Andromedæ, FL. 57. Supra pedem sinistrum.

Aug. 25, Double. Very unequal. L. reddish w.; S. fine  
1779. light sky-blue, inclining to green. Distance  $9''$ ,254 a mean of two years observation. Position  $19^\circ 37'$  n. following. A most beautiful object.

6.  $\beta$  Cephei, FL. 8. In cingulo ad dextrum latus.

Aug. 31. Double. Very unequal. L. blueish w.; S. gar-  
1779. net. Distance  $13''$ ,125. Position  $15^\circ 28'$  f. preceding.

7.  $\beta$  Scorpiæ, FL. 8. Trium in fronte, lucidarum, borea.

Sept. 19, Double. Very unequal. L. whitish r; S. r.  
1779. Distance  $14''$ ,375. Position  $64^\circ 51'$  n. following.

† In a future collection this will be found as a treble star of the first class; the large star having a small one preceding, easily seen with 460 and 932.

8. \*  $\pi$  Bootis, FL. 29.

Sept. 20, Double. Pretty unequal. L. w.; S. w. inclining  
1779. to r. Distance  $6''$ , 171. Position  $6^{\circ} 28'$  f. following.

9. +  $\gamma$  Arietis, FL. 5. Quæ in cornu duarum præcedens.

Sept. 27, Double. Equal, or if any difference the following  
1779. is the largest. Distance  $10''$ , 172, a mean of two  
years observation. L. w. inclining a little to r.; S.  
w. Position  $86^{\circ} 5'$  n. preceding.

10. \*  $\gamma$  Delphini, FL. 12. Borea sequentis lateris, quadrilateri.

Sept. 27, Double. Nearly equal, the following a little  
1779. larger. Both w. Distance  $11''$ , 822, being a mean of  
the measures taken in Sept. Oct. Nov. and Dec. 1779.  
As I suspect a motion in one of these stars, I thought  
it best not to join other observations in that measure.  
Position  $4^{\circ} 9'$  n. preceding.

11.  $\alpha$  Bootis, FL. 17. Trium in sinistro manu præcedens.

Sept. 27, Double. Very unequal. L. w.; S. d. Distance  
1779.  $12'' 503$ , a mean of the observations in 1779, 80, 81.  
Position about  $30^{\circ}$  f. preceding.

12.  $\iota$  Orionis, FL. 44. Trium contiguarum in ense austrina.

Oct. 7, Treble. It is the following or largest of the two  
1779.  $\iota$ s. One is L.; the other two are extremely small.  
L. w.; the other two both dusky r. Distance of  
the nearest  $12'' 5$ . Distance of the farthest  $48'' 31''$ .  
Position of the nearest  $43^{\circ} 51'$  following. Position  
of the farthest  $11^{\circ} 19'$  f. following.

13. and 14.  $\iota$  Orionis, FL. 44. Trium contiguarum in ense  
austrina.

Oct. 7, Double-treble. It is the preceding or smallest of  
1779. the two  $\iota$ s. The preceding set (forming a triangle)  
consists of three equal stars. All dusky r. Distance

- of the two nearest, with 227, about 3 diameters. The following set (forming an arch) consists of three stars of different sizes. The middle star is the largest; that to the south is also pretty large; and the third is very small. L. w.; l. w.; S. pale r. Distance 36'', 25.
15. \*  $\mu$  Cygni, FL. 78.  
Oct. 19, Double. Considerably unequal. L. w.; S. blueish.  
1779. Distance 6'', 927 mean measure. Position  $20^{\circ} 15'$  f. following.
16. \* In constellatione Delphini, FL. 1.  
Nov. 15, Double. It is the star south preceding  $\alpha$ . A little.  
1779. unequal. Both w. Distance 12'', 5. Position  $9^{\circ} 42'$  f. preceding.
17. In extremitate caudæ Lacertæ, FL. 1.  
Nov. 20, Double. Considerably unequal. L. w.; S. d.  
1779, inclining to r. Distance 13'' 43''' inaccurate. Position  $76^{\circ} 16'$  f. preceding.
18. +  $\gamma$  Virginis, FL. 29. De quatuor in ala sinistra, sequens.  
Jan. 21, Double. Equal. Both w. Distance 7'', 333 mean.  
1780. measure. Position  $40^{\circ} 44'$  f. following.
19. +  $\zeta$  Cancræ, FL. 16.  
April 5, Double. Considerably unequal. L. pale r.; S.  
1780. pale r. Distance 8'', 046 mean measure. Position  $88^{\circ} 16'$  f. preceding. See the 24th in the first class.
20. In constellatione Bootis.  
June 25, Double. Draw a line through  $\pi$  and  $\zeta$  to the small  
1780. star under the right foot, and erecting a perpendicular towards the left foot of equal length, the end of it will mark out this double star. Pretty unequal.  
Both

Both r. Distance  $7'' 36'''$  full measure. Position  $59^{\circ} 32'$  n. preceding.

21. In constellatione Equulei, FL. 1.

Aug. 2, Double. Considerably unequal. L. w.; S. much  
1780. inclining to r. Distance  $9'' 375$  mean measure.  
Position  $5^{\circ} 39'$  n. following. A third small star follows at some distance.

22. Quæ infra oculum Lyncis, FL. 12.

Aug. 7, Double. With 222, about 3 diameters of L.  
1780. Considerably unequal. L. w.; S. pale r. Distance  $9'' 23'''$ , not extremely accurate. Position  $32^{\circ} 33'$  n. preceding. See the sixth star in the first class.

23. In constellatione Cassiopeæ, FL. 34.

Aug. 8, Double. It is one of two telescopic stars, and is  
1780. marked  $\phi$  in HARRIS's maps. Extremely unequal.  
L. pale r.; S. d. Distance about  $12''$  or more.

24.  $\theta$  Sagittæ, FL. 17.

Aug. 8, Treble. The two nearest extremely unequal. L.  
1780. pale r.; S. d. Third star pale r. Distance of the two nearest  $11'' 6'''$ . Distance of the two largest  $1' 7'' 49'''$ .

25. In constellatione Serpentarii, FL. 39.

Aug. 24, Double. It is the most south and largest of two  
1780. in the finder. Very unequal. L. w.; S. inclining to blue. Distance  $10'' 2'''$ , a little inaccurate. Position  $87^{\circ} 14'$  n. preceding.

26. \* In constellatione Cerberi L. HEVELII F. FL. Herculis

95.

Sept. 8, Double. It is the star in the leaf nearest to Hercules's face and hand. Equal. Preceding w. Following.

- lowing blueish w. Distance  $6'' 6'''$ . Position  $4^{\circ} 9'$  f. preceding or n. following.
27. In constellatione Navis, near FL. 3.  
Feb. 15, Double. It is a star between  $\gamma$  Canis majoris and  
1781.  $\xi$  Navis. Equal. Distance about  $15''$ .
28. In constellatione Navis, near FL. 9.  
Feb. 15, Double. It is one of two telescopic stars under  
1781. Monoceros. Distance about  $8''$ .
29. In naribus Monocerotis, FL. 8. ::  
Feb. 15, Double. Distance about  $12''$ .
30. \* In constellatione Leonis, FL. 54. Duarum supra dorsum sequens.  
Feb. 21, Double. Considerably unequal. L. brilliant w.;  
1781. S. ash-colour, or greyish w. Distance  $7'' 6''$  mean measure. Position  $9^{\circ} 14'$  f. following.
31. In constellatione Herculis.  
May 20, Double. Over  $\epsilon$  :: Equal. Both very small.  
1781. Distance about  $10''$ .
32. In constellatione Aquilæ, FL. 11.  
July 25, Double. It is the most south of two near  $\epsilon$  and  $\zeta$ .  
1781. Excessively unequal. S. hardly visible with 227, but pretty strong with 460. Distance about  $7''$ .
33. In constellatione Aquilæ, near FL. 7. and 8.  
July 30, Double. It is a star preceding the two small stars  
1781. north of  $k$  and  $l$ . Unequal. L. w.; S. blueish w. Distance  $11'' 35'''$  inaccurate, but not much.
34. In constellatione Aquarii, FL. 94.  
Aug. 20, Double. Between  $\psi$  and  $\omega$  towards  $\delta$ . Very unequal.  
1781. Distance  $13'' 45'''$ . L. pale r.; S. d.
35. In constellatione Serpentarii, FL. 54.  
Aug. 24, Double. It is the preceding of two stars in the head.

1781. head. Excessively unequal. L. reddish w.; S. d. Distance about 8".
36. In constellatione Persei.  
Sept. 14. Double. A little south of  $\gamma$ . Considerably unequal. L. w.; S. w. inclining to r. Distance 11" 53", rather full measure.
37. and 38. In constellatione Persei, near FL. 38 †.  
Sept. 24. Double-double. South preceding the first  $\alpha$ . The 1781. equal set with 227, about 4 or 5 diameters. The unequal set about 5, or 6 diameters. Near this last set is also a third star forming an obtuse angle with the stars of this set. Distance about 10".
39.  $\alpha$  Persei, FL. 40.  
Sept. 24. Double. It is the second or most northern  $\alpha$ . Extremely unequal. L. w.; S. d. With 227, S. is hardly visible; with 460, it appears at first sight. Distance 14" 59", inaccurate on account of the obscurity of S.
40. In constellatione Herculis, near FL. 87.  
Oct. 10. Double. Of three stars, forming an obtuse angle, 1781. whereof FL. 87. (a star south of  $\mu$ ) is at the angular point; that towards Ramus Cereb. Extremely unequal. L. w.; S. d. Distance 10" 20". Position 19° 37' f. following.
41. \* i Herculis, FL. 43.  
Oct. 10. Double. Equal. Preceding star w. A little inclined, to r. Following w. Distance 11" 43". Position 88° 23' n. following.
42. In constellatione Trianguli.  
Oct. 10. Double. It is a star north following  $\delta$ . Unequal. L. 1781. reddish. S. blueish. Both d. Distance about 6 or 7".

† Mr. BRYANT of Bath first observed these stars.

## 43. In sinistro anteriore pede Monocerotis.

Oct. 20, Double. It is the most south of two telescopic  
1781. stars preceding the treble star. Extremely unequal.  
L. w.; S. d. Position  $23^{\circ} 39'$  n. preceding.

## 44. In ore Monocerotis.

Oct. 20, Double. Considerably unequal. L. w.; S. r.  
1781. Distance  $12'' 30'''$ . Position  $60^{\circ} 14'$  n. following.

## 45. In constellatione Tauri, near FL. 10.

Oct. 22, Double. It is near the star sub pede et scapula  
1781. dextra. Extremely unequal. L. pale r.; S. d. Posi-  
tion  $35^{\circ} 33'$  s. preceding.

## 46. In constellatione Monocerotis:

Oct. 22, Double. It is the star following the tip of the  
1781. ear.

## FOURTH CLASS OF DOUBLE STARS.

1.  $\alpha$  Ursæ minoris, FL. 1. Stella Polaris.

Aug. 17, Double. Extremely unequal. L. w.; S. r.  
1779. Distance  $17'' 15'''$ . Position  $66^{\circ} 42'$  s. preceding.

2.  $\gamma$  Lyrae, FL. 20. Duarum contiguarum ad ortum a testa, borea.

Aug. 29, Double. Considerably unequal. L. w.; S. r.  
1779. Distance  $25'' 42'''$ . Position  $31^{\circ} 51'$  s. preceding.  
Three other stars in view.

3.  $\xi$  Capricorni, FL.

Sept. 19, Double. It is the preceding star of two. Ex-  
1779. tremely unequal. Distance about  $25''$ .

4.  $\gamma$  Persei, I. HEVELII 9. In dextro brachio.  
 Sept. 29. Double. Very unequal. L. r.; S. blue. Distance 26'', very inaccurate. Position 39° 5' n. preceding.  
 1779.
5. In constellatione Arietis, FL. 33. Quatuor inform. sup. dors. præc.  
 Sept. 27. Double. It is the first in the head of the fly. L. w.; S. d. Considerably unequal. Distance 25'' 32'' inaccurate. Position 87° 14'.
6.  $\theta$  Serpentis, FL. 63. In extremitate Caudæ.  
 Oct. 17. Double. Equal. Both w. Distance 19'' 375.
7.  $\psi$  Draconis, FL. 31. Prima ad  $\psi$ .  
 Oct. 19. Double. Pretty unequal. L. w.; f. pale r. Distance 28'' 14'',  
 1779.
8. \*  $\zeta$  Piscium, FL. 86. Trium in lino lucidarum sequens.  
 Oct. 19. Double. Pretty unequal. L. w.; S. w. inclining to blue. Distance 22'' 187, not very accurate. Position 22° 37' n. following.
9. \* Prima ad  $\psi$  Piscium, FL. 74. Trium in pinna costarum præcedens.  
 Oct. 30. Double. Distance 27'' 5. Position about 80° f. following. An obscure star also within  $\frac{1}{2}$  minute.  
 1779.
10.  $\chi$  Tauri, FL. 59. Australis sequentis lateris quadrilateri, in cervice.  
 Oct. 30. Double. Distance 18'' 75, very inaccurate.
11.  $\chi$  Cygni, FL. 17.  
 Nov. 20. Double. Very unequal. L. w.; S. dusky r. Distance 24'' 52''.
12. \*  $\psi$  Aquarii, FL. 91.  
 Nov. 26. Double. It is the first of three  $\psi$ 's. Unequal. Distance 23'' 5'', pretty accurate.  
 1779.



13. In constellatione Leonis, FL. 83.

April 6, Double. It is a small star north preceding  $\tau$ . A  
1780. little unequal. Both inclining to r. Distance  
29'' 5'''. Position  $54^{\circ} 55'$  f. following.

14. In constellatione Aquilæ, FL. 57.

Aug. 2, Double. It is the preceding of two, near the  
1780. south end of Antinous's bow. A little unequal. L.  
w.; S. w. inclining to r. Distance 29'' 28''', pretty  
accurate. Position  $81^{\circ} 55'$  f. preceding.

15. In dextra aure Camelopardali. I. HEVELII ultima.

Aug. 2, Double. A little unequal. L. reddish w.; S.  
1780. reddish w. Distance 20'' 5'''.

16. In constellatione Cassiopeæ, FL. 31.

Aug. 2, Double. It is marked with the letter A in HAR-  
1780. RIS's maps. Distance about 20'' or more.

17. \* Cor Caroli, FL. 12. Canum Venaticorum.

Aug. 7, Double. Very unequal. L. w.; S. inclining to r.  
1780. Distance 20'' 0''', inaccurate. Position  $41^{\circ} 47'$  f.  
preceding.

18. \* In constellatione Cygni, FL. 61.

Sept. 20. Double. It is a star preceding  $\tau$ . Pretty unequal.  
1780. L. pale r.; S. r.; or L. r.; S. garnet. Distance  
16'' 7'''. Position  $36^{\circ} 28'$  n. following.

19. In constellatione Aurigæ, FL. 14.

Sept. 24, Double. It is the preceding star of a cluster of  
1780. stars that precede  $\phi$  and  $\chi$ . Very unequal. L. red-  
dish w.; S. d. Distance 16'' 8''', a little inaccurate.  
Position  $37^{\circ} 38'$  f. preceding.

20.  $\delta$  Draconis, FL. 47.

Oct. 3, Double. Very unequal. L. pale r.; S. dusky r.  
1780. Distance 26'' 39''. Position  $90^{\circ}$  n. preceding or fol-  
lowing, by exact estimation.

21.  $\zeta$  Orionis, FL. 50. Trium in cingulo sequens.  
 Oa. 10, Double. Very unequal. L. w.; S. d. Distance  
 1780. about  $25''$ . Position  $83^\circ 25'$  n. following, very  
 inaccurate.
22.  $f$  Cygni, FL. 63. ::  
 Oa. 27, Double. Extremely unequal. L. fine w.; S. d.  
 1780. Distance  $18'' 11'''$ .
23. 2 ad  $\omega$  Cygni, FL. 45. In genu dextro.  
 Oa. 27, Double. Considerably unequal. L. reddish w.;  
 1780. S. d. Distance within  $30''$ . Position  $7^\circ 23'$  n. pre-  
 ceding.
24. 3 ad  $\omega$  Cygni, FL. 46. In genu dextro.  
 Oa. 27, Treble. Very unequal, and extremely unequal.  
 1780. L. fine garnet; S. r.; smallest d. All within  $30''$ .  
 Position of the brightest of the two small stars  
 $44^\circ 19'$  n. preceding. Position of the faintest —  
 preceding.
25. In constellatione Ceti.  
 Dec. 23, Double. It is a star near the place of the periodi-  
 1780. cal star  $\sigma$ . Distance  $16'' 875$ , a little inaccurate.
26. In constellatione Navis, FL. 19. ::  
 Feb. 15, Double. It is a star under the ham of Mono-  
 1781. ceros's right-foot. Distance about  $25''$ .
27. In constellatione Comæ Berenices, FL. 24.  
 Feb. 28, Double. Considerably unequal. L. whitish r.;  
 1781. S. blueish r. Mean distance  $18'' 24'''$ . Position  
 $3^\circ 28'$  n. preceding.
28. In constellatione Geminorum.  
 Mar. 23, Double. It is near  $\gamma$  towards  $\zeta$  Tauri. A little  
 1781. unequal. Both r. Distance  $19'' 41'''$ . Position  
 $57^\circ 0'$  f. preceding.

29. *b* Ursæ majoris, FL. 23. Duarum in collis sequens.

Apr. 25, Double. Extremely unequal. L. reddish w. ;  
1781. S. d. Distance with 460,  $19'' 14'''$ . Position  $3^{\circ} 14'$   
n. preceding.

30. In constellatione Lyncis, FL. 44.

May 26, Double. It is the eye of nose of Leo minor.  
1781. Unequal. Distance  $24'' 53'''$  inaccurate.

31. In constellatione Cephæi, near FL. 27.

May 27, Treble. It is a star near  $\delta$ . Distance of the nearest  
1781. about  $20''$ .

32. \* In constellatione Serpentarii, FL. 61.

July 15, Double. It is a star near  $\gamma$ . A little unequal.  
1781. L. w. ; S. grey. Distance  $19'' 4'''$ , inaccurate. Po-  
sition almost directly following.

33. In constellatione Aquilæ.

July 19, Treble. It is the first of two stars preceding  $v$ .  
1781. Distance of the two nearest  $21'' 59'''$ , inaccurate.

34. In constellatione Aquilæ, near FL. 64.

July 25, Double. It is near a star preceding  $\theta$ . Equal  
1781. distance about  $30''$ .

35.  $\beta$  Delphini, FL. 6. Austrina præcedentis lateris quadri-  
lateri.

Aug. 1, Double. Extremely unequal. Hardly visible with  
1781. 227; pretty strong with 460. Distance  $25'' 54'''$ ,  
rather narrow measure. Position  $79^{\circ}$  n. preceding;  
by exact estimation.

36.  $\beta$  Serpentis, FL. 28. In educatione colli.

Aug. 13, Double. Extremely unequal. L. w. ; S. ex-  
1781. tremely faint. Distance  $24''$ , pretty exactly esti-  
mated. Position 3 or  $4^{\circ}$  f. preceding, too obscure for  
measuring.

37.  $\delta$  Equulei, FL. 7. Duarum in ore sequens.

Aug. 13, Double. Excessively unequal. S. hardly visible  
1781. with 227; but with 460, visible at first sight. L.  
w.; S. d. Distance  $19'' 32'''$ . S. too obscure to be  
very accurate. Position  $11^{\circ} 39'$  n. following.

38. In constellatione Aquarii, FL. 24.

Aug. 14, Double. It is the star in the cheek or hair of the  
1781. neck. Very unequal. L. w.; S. d. Distance  $25''$ ,  
very inaccurate.

39. In constellatione Cygni.

Oct. 1, Double. It is a star north following  $\sigma$ . Extremely  
1781. unequal. L. w.; S. d. Distance  $18''$  exact estima-  
tion. Position  $30^{\circ} 28'$  f. following.

40.  $\alpha$  Trianguli, FL. 10.

Oct. 8, Double. It is the preceding of three telescopic  
1781. stars. Unequal. Distance  $17'' 19'''$ , pretty accu-  
rate.

41.  $\mu$  Herculis, FL. 86.

Oct. 10, Double. Excessively unequal. The small star is  
1781. not visible with 227, nor with 278. I saw it very  
well with 460. L. inclined to pale r.; S. d. Dis-  
tance, by pretty exact estimation,  $18''$ . Position, by  
very exact estimation,  $30^{\circ}$  f. preceding.

42. In constellatione Herculis.

Oct. 10, Double. It is a star just by  $\nu$ . Considerably une-  
1781. qual. L. inclined to r.; S. inclined to blue. Distance  
 $18'' 19'''$ . Position  $4^{\circ} 58'$  n. preceding.

43.  $\lambda$  Eridani, FL. ultima. In origine fluvii.

Oct. 22, Double. It is the middle of three telescopic stars.  
1781. Very unequal. L. w.; S. r.

44. In

44. In constellatione Tauri, near FL 4.

Dec. 22, Double. It is a small telescopic star south following s. Extremely unequal. L. w.; S. d.  
1781.

# FIFTH CLASS OF DOUBLE STARS.

1.  $\delta$  Herculis, FL. 11. In sinistro humero.

Aug. 9, Double. Extremely unequal. L. w.; S. inclining to r. Distance  $33''$ , 75. Position  $72^\circ 28'$  f. following.  
1779.

2. \*  $\zeta$  Lyræ, FL. 6.

Aug. 29, Double. Pretty unequal. L. w.; S. w. inclining to pale rose colour. Distance  $41''$  58''', perhaps a little inaccurate. Position  $62^\circ 18'$  f. following, a little inaccurate.  
1779.

3. \*  $\beta$  Lyræ, FL. 10. Duarum in jugimento borea.

Aug. 29, Quadruple. All w. First and second considerably unequal. First and third very unequal. First and fourth very unequal. The second a little inclining to r. The third and fourth more inclining to r. Distance of the first and second  $43''$  57'''. Position  $60^\circ 28'$  f. following, a little inaccurate.  
1779.

4.  $\delta$  Cephei, FL. 27. Sequitur tiaram.

Aug. 31, Double. Considerably unequal. L. reddish w.; S. blueish w. Distance  $38''$  18''', a bright object.  
1779.

5. +  $\beta$  Cygni, FL. 6. In ore.

Sept. 12, Double. Considerably unequal. L. pale r.; S. a beautiful blue. The estimation of the colours the same  
1779.

same with 227 and 460. Distance  $39'' 32'''$ , pretty accurate. Position  $36^{\circ} 28'$  n. following.

6. \*  $\nu$  Scorpii, FL. 14. Duarum adjacentium boreæ frontis, borea.

Sept. 19, Double. Very unequal. Both w. Distance  
1779.  $38'' 20'''$ , pretty accurate. Position  $69^{\circ} 28'$  n. preceding.

7.  $\mu$  Sagittarii, FL. 13. In summo arcu, borealis.

Sept. 19, Treble. Two small stars near on each side.  
1779. L. w.; S. both r. Distance of the nearest about  $30''$ . Position — preceding, the other — following.

8.  $\alpha$  Herculis, FL. 7. In dextri brachii ancone.

Sept. 20, Double. A little unequal. L. r.; S. garnet; or  
1779. L. pale r.; S. r. (when the stars are low the first estimation of the colours will take place). Distance  $39'' 59'''$ . Position  $79^{\circ} 37'$  n. following. Has altho a third star.

9.  $\gamma$  Bootis, FL. 21. Trium in sinistra manu, media.

Sept. 27, Double. Very unequal. L. w.; S. d. Distance  
1779.  $37'' 56$ . This is not a mean of the measures; for I suspect a motion in one of the stars, which another year or two may shew. Position  $52^{\circ} 51'$  n. following.

10. \*  $\delta$  Orionis, FL. 34. Trium in cingulo præcedens.

Oa. 6, Double. Considerably unequal. L. w.; S. blueish  
1779. r. Distance  $52'' 968$  full measure. Position  $88^{\circ} 10'$  n. preceding.

11. +  $\nu$  Draconis, FL. 24. and 25. In ore duplex.

Oa. 19, Double. A little unequal. L. pale r.; S. pale r.  
1779. Distance  $54'' 48'''$ . Position  $44^{\circ} 19'$  n. preceding.

From the right ascension and declination of these stars in FLAMSTEAD's catalogue we gather, that in  
his

his time their distance was  $1' 11''$ , 418; their position  $44^{\circ} 23'$  n. preceding; their magnitude equal or nearly so. The difference in the distance of the two stars is so considerable, that we can hardly account for it otherwise than by admitting a proper motion in either one or the other of the stars, or in our solar system; most probably neither of the three is at rest.

12. \*  $\lambda$  Arietis, FL. 9. In vertice.

Oct. 30. Double. Considerably unequal. L. pale r.; S. dusky garnet. Distance  $36'' 44'''$ , a little inaccurate. Position  $42^{\circ} 0'$  n. following.

13.  $\phi$  Tauri, FL. 52. Borea sequentis lateris quadrilateri in Cervice.

Oct. 30. Double. Distance  $55'' 6\frac{1}{2}''$ , inaccurate.

14. In constellatione Monocerotis.

Dec. 5. Multiple. It is a spot over the right fore-foot;  
1779. 4 or 5 small stars within one minute.

15.  $\epsilon$  Ursæ majoris, FL. 16.

May 2. Double. Very unequal. L. whitish r.; S. d.  
1780. Distance with 460,  $48'' 59'''$ . Position  $80^{\circ} 47'$  f. preceding.

16.  $\sigma$  Piscium, FL. 76. Duarum in ore piscis sequentis borealior.

Aug. 3. Double. Extremely unequal. L. pale r.; S. dusky r. Distance  $48'' 125$ , pretty accurate. Position  $15^{\circ} 28'$  n. preceding.

17.  $\pi$  Andromedæ, FL. 29. In dextro humero.

Aug. 25. Double. Extremely unequal. L. w.; S. blueish.  
1780. Distance  $34'' 12'''$ , inaccurate.

18.  $\alpha$  Cassiopeæ, FL. 18. In pectore.

Aug. 31, Double. Extremely unequal. L. pale r.; S. d.

1780. Distance  $52''$ , 812. Position  $40^\circ 58'$  n. preceding.

19.  $\gamma$  Herculis, FL. 20. In dextro brachio.

Sept. 4, Double. Extremely unequal. L. reddish w.; S.

1780. r. Distance  $41'' 49'''$ , a little inaccurate. Position  $19^\circ 30'$  f. preceding.

20.  $\epsilon$  Pegasi, FL. 1.

Sept. 8, Double. Very unequal. L. pale r.; S. d.; Dis-

1780. tance  $37'' 5'''$ , pretty accurate. Position  $38^\circ 19'$  n. preceding.

21.  $\tau$  Aurigæ, FL. 29.

Sept. 26, Double, about  $30''$ .

22.  $\lambda$  Aurigæ, FL. 15.

Sept. 30. Multiple. Two are within about  $30''$ .

23. In constellatione Orionis.

Oct. 10, Double. It is a star following f. Distance about

1780.  $40''$

24. In constellatione Ceti, FL. 37.

Oct. 12. Double. It is a star between  $\eta$  and  $\theta$  towards the

1780. north. Distance  $42''$ , 812, inaccurate.

25.  $\tau$  Orionis, FL. 20. supra talum in tibia.

Oct. 23, Double. Very unequal. Distance about  $30''$

26.  $b$  Leonis, FL. 6.

Feb. 21, Double. Very unequal. L. r.; S. d. Distance

1781.  $35'' 48'''$ . Position  $12^\circ 55'$  n. following.

27. In constellatione Libræ, near FL. 31.

May. 24, Double. The most south of three small stars in

1781. the finder. Equal, or the preceding rather the largest. Both w. inclining to pale r. Distance  $44'' 12'''$ , a little inaccurate. Position  $40^\circ 17'$  f. following.



28. In constellatione Cephei.

May. 27, Double. It is a star near  $\beta$ . Extremely unequal.  
1781. Distance about 30".

29.  $\gamma$  Serpentis, FL. 53. Post dextrum femur Serpentarii.

July 16. Double. Unequal. Distance about 35".

30. In constellatione Serpentarii, FL. 53.

July 19, Double. It is a star between  $\alpha$  and  $\beta$  one-third  
1781. of the way from  $\alpha$ . Very unequal. L. w.; S.  
inclining to r. Distance 32" 21", narrow mea-  
sure.

31. In constellatione Aquilæ.

July 19, Double. It is the star next but one preceding  $\delta$ .  
1781. Very unequal. L. r.; S. d. Distance about 30".

32.  $\alpha$  Andromedæ.

July 21, Double. Extremely unequal. The small star  
1781. better with 460 than with 227. L. w.; S. d. Dis-  
tance 55" 32", rather narrow measure. Position  
10° 37' f. preceding.

33.  $b$  Aquilæ, FL. 15.

July 25, Double. Unequal. Both pale r. Distance 33" 53",  
1781. inaccurate.

34. In constellatione Aquilæ, near FL. 28.

July 25, Double. It is one of two stars near A. Distance  
1781. about 35".

35. In constellatione Aquilæ.

July 25. Double. It is a star near that which follows  $\theta$ .  
1781. Very unequal. Distance about 40".

36.  $\sigma$  Scuti, FL. 2. in constellatione Aquilæ.

July 30, Double. Very unequal. L. pale r.; S. d. Dis-  
1781. tance 42" 44", a little inaccurate.

37.  $\nu$  Coronæ, FL. 18.

Sept. 21, Treble. Very unequal. L. w.; S. both r. Distance of the nearest about  $50''$ ; the  $1\frac{1}{2}$  min. †

38. In constellatione Herculis; FL. 23.

Sept. 21, Double. It is the star between  $\nu$  and  $\xi$  Coronæ, 1781. the largest of a telescopic triangle. Distance  $36'' 27'''$ , rather narrow measure. L. w.; S. w. inclining to r.

39.  $\alpha$  Lyræ, FL. 3. In testa fulgida.

Sept. 24, Double. Excessively unequal. By moon-light I 1781. could not see the small star with 278, and saw it with great difficulty with 460; but in the absence of the moon I have seen it very well with 227. L. fine brilliant w.; S. dusky. Distance  $37'' 13'''$ . Position  $26^\circ 46'$  f. following.

Oct. 22, Having often measured the diameters of many of 1781. the principal fixed stars, and having always found that they measured less and less the more I magnified, I fixed upon this fine star for taking a measure with the highest power I have yet been able to apply, and upon the largest scale of my new micrometer I could conveniently use. With a power of 6450 (determined by experiments upon a known object at a known distance) I looked at this star for at least a quarter of an hour, that the eye might adapt itself to the object; having experimentally found, that the aberration by this means will appear less and less, and, in the telescope I used upon this occasion with powers from 460 to 1500, will often quite vanish, and

† In a future collection the small star at the obtuse angular point will be found as a double star of the second or third class.

leave a very well-defined circular disk for the apparent diameter of the stars. The diameter of  $\alpha$  Lyræ, by this attention, appeared perfectly round, and occasionally separated from rays that were flashing about it. From the very brilliant appearance of the star with this great power, and a pretty accurate rough calculation founded on its apparent brightness, when observed with the naked eye with 227, with 460, with 6450, I surmise, that it has light enough to bear being magnified at least a hundred thousand times with no more than six inches of aperture, provided we could have such a power, and other considerations would allow us to apply it. When I had as good a view as I expected to have, I took its diameter with my new micrometer upon a scale of eight inches and 4428 ten thousandth to  $1''$  of a degree, and found it subtended an angle of  $0'',3553$ . I had no person at the clock; but suppose the time of its passing through the field of my telescope (which in this great power is purposely left undefined, and as large as possible) was less than three seconds.

40.  $\gamma$  Lyræ, FL. 8.

Sept. 24, Treble. Extremely unequal. L. w.; S. both d.  
1781. One n. preceding, the other s. following. Distance of the following star  $56'' 47'''$ , a little inaccurate. Position of the same  $28^\circ 27'$  s. following.

41. A Persei, FL. 43.

Sept. 24. Double. Unequal. L. w. Distance about  $50''$ .

42. In constellatione Lyræ.

Sept. 25, Double. It is a small star just by  $\gamma$ . A little unequal

1781. equal. Both r. Distance  $38'' 8'''$ . Position  $26^{\circ} 18'$  n. following.
43. In constellatione Cygni, FL. 76.  
 Oa. 1, Double. It is the third star from  $\rho$  towards  $\nu$ .  
 1781. Unequal. Distance  $48''$  by exact estimation. Position — preceding.
44. In constellatione Cygni, FL. 69.  
 Oa. 1, Treble. Very unequal. L. w.; S. both reddish.  
 1781. Position both — preceding.
45. In constellatione Cygni.  
 Oa. 1, Double. It is the most south of two telescopic stars.  
 1781. following  $\tau$ . Very unequal. L. w.; S. d. Distance  $44''$  by exact estimation. Position — following.
46. c Cygni, FL. 16. 1<sup>a</sup> ad c.  
 Oa. 5, Double. It is the star next following  $\theta$ . Almost  
 1781. equal. Both pale r. Distance  $30''$  by pretty exact estimation.
47. c Cygni, FL. 26. 2<sup>a</sup> ad c.  
 Oa. 8, Double. Very unequal. L. reddish w.; S. dusky  
 1781. r. Distance  $39''$  by pretty exact estimation.
48. \* In constellatione Piscium.  
 Oa. 8, Double. It is a telescopic star just by  $\theta$  north-  
 1781. wards. Both d. Distance about  $45''$ .
49. \* In constellatione Arietis, FL. 30.  
 Oa. 15, Double. It is a small star over the Ram's back  
 1781. Nearly equal. Distance  $31'' 6'''$ , inaccurate.
50.  $\gamma$  Leporis, FL. 13. In posterioribus pedibus austrina.  
 Oa. 22, Double. Considerably unequal. Distance about  $40'$ .
51. In constellatione Sagittæ.  
 Nov. 23, Double. It is a star north following  $\epsilon$ . Extremely  
 1781. unequal. Distance  $32'' 48'''$ . L. r.; S. blue.

## SIXTH CLASS OF DOUBLE STARS.

1.  $\alpha$  Ceti, FL. 68. In pectore nova.

Oct. 20. Double. Very unequal. L. garnet. S. dusky.

1777. Dist.  $\left\{ \begin{array}{l} \text{mean of some very accurate measures } 1'44'', 218 \\ \text{mean of other very accurate measures } 1'53'', 032. \end{array} \right.$

As I can hardly doubt the motion of this star, I have given the mean of the most accurate measures separately; and hope in a few years time to be able to give a better account of it.

2.  $\alpha$  Serpentarii, FL. 67.

Aug. 29, 1779. Double. Distance about  $1\frac{1}{4}$  min.

3.  $\delta$  Lyræ, FL. 11.

Aug. 29, Double. Extremely unequal. L. w.; S. d. Distance about  $4'$ , pretty exact estimation.

4.  $\alpha$  Capricorni, FL. 5.

Sept. 19, Double. Very unequal. L. r.; S. d. Distance about  $1\frac{1}{4}$  min. Position — f. preceding.

5. In constellatione Arietis, FL. 35. supra dorsum.

Sept. 27, Double. It is the star in the body of the fly. Distance  $2' 5'' 35'''$ .

6.  $\epsilon$  Capricorni, FL. 39. Duarum in educatione caudæ præced.

Sept. 27, Double. Unequal. L. pale r. Distance about  $1\frac{1}{4}$  min.

7.  $\ast \tau$  Tauri, FL. 94. In educatione cornu borei.

Oct. 6. Double. Distance  $1' 11'', 25'''$ , pretty accurate.

8.  $\kappa$  Tauri, FL. 59.

Oct. 6. Double. At a considerable distance.

9. \* ζ Geminorum, FL. 43. In sinistro genu sequentis II.  
Oa. 7, Double. Very unequal. L. reddish w.; S. dusky r.  
1779. Distance 1' 31" 52"', rather full measure. Position  
81° 14' n. preceding.
10. o Cygni, FL. 31. Duarum in dextro pede sequens.  
Nov. 2, Double. Considerably unequal. L. pale r. S.  
1779. blue. It is the following star of the two o's that are  
close together. Distance 1' 39" 57'''. Position  
87° 14' f. preceding.
11. \* α Leonis, FL. 32. In corde.  
Nov. 14, Double. Very unequal. L. w.; S. d. Distance  
1779. 2' 48" 20'''. Position 30° 5' n. preceding.
12. \* γ Leonis, FL. 84. Quasi in cubito.  
April 6, Double. Considerably unequal. L. r.; S. in-  
1780. clining to blue. Distance 1' 22" 42'''. Position  
73° 29' f. following.
13. o Leonis, FL. 95. In extremitate caudæ.  
April 6, Double. Extremely unequal. L. reddish w.; S.  
1780. d. Distance about 1½ min. Position about 80° n. fol-  
lowing.
14. η Serpentis, FL. 58. In cauda.  
June 19, Double. Extremely unequal. L. pale r.; S. d.  
1780. Distance 1' 21" 2'''. Position 9° 7' f. following.
15. In constellatione Bootis, near FL. 6.  
June 25, Double. It is a telescopic star near that which  
1780. forms a rectangle with α and γ. Distance about 2'.
16. δ Bootis, FL. 49. In dextro humero.  
July 23, Double. Considerably unequal. Distance about  
1780. 2½ min. L. reddish w.; S. w. Position 5° 46' n.  
following.

17.  $\mu$  Bootis, FL. 51. In baculo recurvo.

July 30, Double. Unequal. Distance 2' 8'', exact estimation.  
1780. Position  $80^{\circ} 25'$  s. following. L. reddish w.  
S. pale r. See the 17th star of the first class.

18.  $\nu$  Coronæ, FL. 21.

July 30, Double. Very unequal. L. r.; S. garnet. At  
1780. some considerable distance. Position about  $80^{\circ}$  n.  
following.

19.  $\chi$  Persei.

Aug. 2, Multiple. An astonishing number of small stars  
1780. all within the space of a few minutes. I counted not  
less than 40 within my small field of view.

20.  $\mu$  Persei, FL. 51. Duarum in dextro poplite sequens.

Aug. 2, Double. Very unequal. L. w. Distance about  $1\frac{1}{4}'$ .

21.  $\eta$  Pegasi, FL. 44.

Aug. 23. Double. Distance about  $2\frac{1}{4}$  min.

22. In constellatione Draconis, I. HEVELII 69.

Aug. 7, Double. It is the star between  $\alpha$  Draconis and  
1780. the tail of Urta major. Distance about  $3\frac{1}{2}$  min.

23. In naribus Lyncis.

Aug. 7. Double. Distance about 2'.

24.  $\delta$  Cassiopeæ, FL. 4.

Aug. 12, Treble. Two are large. Distance about 2'. A  
1780. third is obscure. Distance about  $1\frac{1}{4}$  min. They  
form almost a rectangle.

25. In constellatione Cassiopeæ, FL. 3.

Aug. 18. Double. Distance about  $2\frac{1}{4}$  min.

26.  $\epsilon$  Sagittæ, FL. 11.

Aug. 19, Double. Very unequal. L. r.; S. r. inclining to  
1780. blue. Distance  $1' 31'' 53'''$ . Position  $8^{\circ} 32'$  s.  
following.

27. In constellatione Aquilæ.

Aug. 24. Double. It is a star north of  $\theta$ . Distance about 1'.

28.  $\beta$  Capricorni, FL. 9. Trium in sequente cornu austrina.

Aug. 26, Double. Considerably unequal. Distance about 1780. 3'. Position — preceding.

29.  $\pi$  Capricorni, FL. 10. Trium in rostro præcedens.

Aug. 26. Double. Distance about  $2\frac{1}{2}$  min.

30.  $\alpha$  Aurigæ, FL. 13. In humero sinistro.

Sept. 8, Double. Extremely unequal. L. w.; S. d.  
1780. Distance 2' 49'' 8'''. Position  $33^\circ 42'$  f. following.  
With a power of 227, and my common micrometer, the diameter of this star measured 2'',5. The circumference was remarkably well defined.

31.  $\delta$  Tauri, FL. 88. In sinistro cubito.

Sept. 24, Double. Distance 1' 10'',625. A little inaccurate.  
1780.

32.  $\lambda$  Cygni, FL. 54.

Sept. 20, Double. Extremely unequal. L. blueish w.; S. d.  
1780. Distance about 1 min. Position  $12^\circ 42'$  f. following.

33. In constellatione Cygni, FL. 32.

Sept. 20. Double. Distance about 2 min.

34.  $\theta$  Aurigæ, FL. 37. In dextro carpo.

Sept. 26. Double. Distance about  $2\frac{1}{2}$  min.

35. In constellatione Camelopardali, FL. 13.

Sept. 26. Double. It is the star over the goat's head. Distance about 2'.

36. In constellatione Camelopardali, FL. 10.

Sept. 30. Double. Distance about  $1\frac{1}{2}$  min.



37.  $\epsilon$  Draconis, FL. 46. In flexura colli.  
Oct. 3. Double. Distance 3 or 4'. A rich spot.
38.  $\epsilon$  Draconis, FL. 64 or 65.  
Oct. 3. Double. Distance about 2'.
39.  $\alpha$  Orionis, FL. 58. In dextro humero lucida rutilans.  
Oct. 10, Double. Extremely unequal. L. r. but not deep;  
1780. S. d. Distance 2' 6" 2". Position  $62^{\circ} 18'$  s. following.
40.  $\gamma$  Leporis, FF. 13.  
Feb. 21, 1781. Double. Distance about  $2\frac{1}{2}$  min.
41.  $\rho$  Cancræ 5 ad  $\rho$ , FL. 67.  
Feb. 21, 1781. Double. Very unequal. L. reddish w.; S. d.  
Distance 1' 35" 59". Position  $50^{\circ} 33'$  n. preceding.
42.  $\beta$  Geminorum, FL. 78. In capite sequentis II.  
Mar. 13, 1781. Multiple. Extremely unequal. The nearest distance 1' 56" 45", rather full measure. Position  $24^{\circ} 28'$  n. following, not extremely accurate. This is the smallest. The next distance 3' 17" 19", pretty accurate. Position  $15^{\circ} 56'$  n. following. A third I did not measure.
43.  $\theta$  Virginis, FL. 51. De quatuor ultima et sequens.  
May 14, 1781. Double. Extremely unequal. L. w.; S. d. Distance 1' 3" 53", inaccurate. Position  $24^{\circ} 55'$  n. preceding.
44.  $\iota$  Libræ, FL. 24.  
May 24, 1781. Double. Very unequal. L. w.; S. dusky r. Distance 1' 5" 10", not accurate. Position  $22^{\circ} 31'$  s. following.
45. In constellatione Andromedæ.  
July 21, 1781. Double. It is a star near  $\iota$  towards  $\sigma$ . L. r. Distance about  $1\frac{1}{2}$  min.

46.  $\alpha$  Aquilæ, FL. 53.

July 23, Double. Extremely unequal. L. w.; S d. Dis-  
1781. tance  $2' 23'' 18'''$ . Position  $64^{\circ} 44'$  n. preceding.

47. In constellatione Aquilæ, near FL. 35.

July 25, Double. It is one of the preceding stars of a  
1781. small quartile near  $c$ , not very near.

48. In constellatione Aquilæ, near FL. 35.

July 25, Double. It is also one of the preceding stars of a  
1781. small quartile near  $c$ , not very near.

49. In constellatione Aquilæ.

July 26. Double. The following star of a trapezium near l.

50. In constellatione Aquilæ.

July 26, Double. The following star of a trapezium near  
1781. l. not near.

51. In monte Mænali Heveliana.

Aug. 5, Double. It is a star near the middle. The fol-  
1781. lowing of two, not very near.

52. In constellatione Bootis.

Aug. 17, Double. It is a star between  $e$  and  $f$ . Distance  
1781. above  $1'$ . Unequal.

53. In constellatione Bootis.

Aug. 17, Double. It is a star more south than  $i$ . Distance  
1781. above  $1'$ .

54. In constellatione Serpentarii.

Aug. 21, Double. It is a star more south than  $o$ . Distance  
1781.  $75''$ , exact estimation.

55. In constellatione Cassiopeæ, FL. 2.

Sept. 6. Double. It is a star near  $e$ . L. r. Dist. within  $2\frac{1}{2}'$ .

56.  $\theta$  Lyræ, FL. ultima.

Sept. 25, Double. Very unequal. L. w.; S. inclining to r.  
1781. Distance about  $1\frac{1}{2}$  min. Position — n. following.

57. In constellatione Cygni, FL. 79.  
 Oa. 1, Double. It is the fifth star from  $\rho$  to  $\nu$ . Unequal.  
 1781. L. w.; S. pale r. Distance  $1' 40''$  estimation.
58. In constellatione Aquarii, FL. 5.  
 Oa. 5, Double. It is the most south of two in the arrow  
 1781. of Antinous. Distance above  $1'$ .
59. In constellatione Cygni, near FL. 28.  
 Oa. 5, Double. It is a star near  $b$ . Distance  $73''$ , exact  
 1781. estimation.
60. In constellatione Cygni.  
 Oa. 8, Double. It is a star near the second  $c$ . Confi-  
 1781. derably unequal. L. w.; S. d. Distance  $88''$ , exact  
 estimation.
61. In constellatione Piscium, near FL. 7.  
 Oa. 8, Treble. It is a star preceding  $b$ . They form a  
 1781. triangle, each side of which is about  $1'$ .
62.  $\alpha$  Piscium, FL. 8. In ventre.  
 Oa. 8. Double. Distance near  $2'$ .
63. In constellatione Sagittæ.  
 Oa. 12, Double. It is near the star north following  $a$ .  
 1781. Extremely unequal. L. w. inclining to r.; S. d.  
 Distance  $1' 30'' 56'''$ . Position  $4^\circ 9'$  f. preceding.  
 A third star in the same direction, at a little more  
 than twice the distance. A fourth star in view.
64. In constellatione Eridani.  
 Oa. 22, Double. It is the small star near  $\nu$ . Distance  
 1781. about  $1\frac{1}{2}$  min.
65. In capite Monocerotis.  
 Oa. 22, Multiple. It is one star with at least 12 around it,  
 1781. all within the field of my telescope.

66.  $\alpha$  Tauri, FL. 87. Splendida in austrina oculo.

Dec. 19, Double. Extremely unequal. L. r.; S. d. Distance 1' 27" 45". Position  $52^{\circ} 58'$  n. following. 1781. With 460, the apparent diameter of this star, when on the meridian, measured 1" 46", a mean of two very complete observations, they agreed to 6"; with 932, it measured 1" 12", also a mean of two excellent observations; they agreed to 8". The apparent disk was perfectly well defined with both powers.

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POSTSCRIPT TO THE CATALOGUE OF DOUBLE STARS.

SINCE my having delivered my paper on the Parallax of the Fixed Stars, in which I refer to the above Catalogue of Double Stars, I have received, by the favour of our President Sir JOSEPH BANKS, the fourth volume of the Acta Academiae Theodoro Palatinæ, which contains a most excellent Memoir of Mr. MAYER's, "De novis in Cœlo fixæ Phænomenis;" wherein I see that the idea of ascertaining the proper motion of the stars by means of small stars that are situated at no great distance from large ones, has induced that gentleman before me to look out for such small stars. In the course of that undertaking he has discovered a good many double stars, of which he has given us a pretty large list, some of them the same with those in my catalogue. My view being the annual parallax required stars much nearer than those that would do for Mr.

MAYER'S

MAYER's purpose; therefore I examined the heavens with much higher powers, and looked out chiefly for such as were exceedingly close.

The above catalogue contains 269 double stars, 227 of which, to my present knowledge, have not been noticed by any person. I hope they will prove no inconsiderable addition to the general stock, especially as in that number there are a great many which are out of the reach of Mr. MAYER's and other mural quadrant or transit instruments. It can hardly be expected, that a power of 70 or 80 would be sufficient to discover those curious stars that are contained in the first class of my catalogue; so that it is not strange they should have entirely escaped Mr. MAYER's notice. We see that it is not for want of his looking at those stars; for we find he has frequently observed  $\zeta$  Cancri, the star near Procyon, and the star in Monoceros, without perceiving the small stars near them, which I have pointed out. Nor is it only in the first class that his telescope wanted power, light, and distinctness; for the small stars that are near  $\beta$  Orionis,  $\beta$  Serpentis,  $\zeta$  Orionis,  $\epsilon$  Pegasi,  $\alpha$  Lyrae,  $\alpha$  Andromedae,  $\mu$  Sagittarii,  $\alpha$  Aquilae,  $\eta$  Pegasi,  $\delta$  Lyrae,  $\epsilon$  Librae,  $\alpha$  Piscium,  $\alpha$  Tauri, and many more, have escaped his discovery, though he has given us the places of other more distant small stars not far from them, and therefore must have had them frequently in the field of view of his telescope. In settling the relative situations of very close double stars, neither Mr. MAYER's instruments, nor his method, were adequate to the purpose. It is well known, that whenever we employ time as a measure, the results cannot be very accurate; because a mistake of no more than a tenth part of a second in time will produce an error of a whole second and an half in measure, so that his *R.* must be

be extremely defective. Nor could his micrometer give the declination much better unless the telescope had bore a power of at least 4. or 500. When the angle of position is but small, such as 3, 4, 5, or 6 degrees, and the distance of the stars not above a few seconds, it is evident, that a micrometer must be able to measure tenths of a second at least to give even a tolerable exactness of position. On the contrary, the position being measured with such a micrometer as I have constructed for the purpose, we may from thence deduce the declination, with great confidence, true to a quarter of a tenth of a second for every second of the distance of the stars.

Mr. MAYER's account of  $\alpha$  Geminorum, for instance, gives a difference of  $5'',7$  of time in  $R$ , of  $3'',8$  in declination, and of 1 to 6 in magnitude or degree of light of the stars. These quantities reduced to my notation, and compared with my measures of the same star, give

Mr. MAYER's	Distance $9'',635$ from center to center	Mine	$5'',156$ diameters included.
	Position $23^{\circ} 14'$ n. preceding		$32^{\circ} 47'$ n. preceding.
	Magnitude extremely unequal		A little unequal.

To account for this difference I ascribe Mr. MAYER's error in distance to his method of measuring by time. The error of position follows always from an observation of the declination taken with the common micrometer, when it is deduced from an erroneous  $R$ . In my measures the distance and position are independent of each other, which I look upon as no small advantage of my cross-hair micrometer. The error in the magnitudes of the stars I ascribe to the want of power in Mr. MAYER's telescope, which did not separate the stars far enough for him to judge accurately of their size, otherwise he would soon have found, that instead of five there is hardly so much as

one

one single degree of difference in their magnitudes. See fig. 6. for a representation of those stars with my power of 460.

I do not mean to depreciate Mr. MAYER's method, the excellence of which is well known; and with some stars of my third, all those of the fourth, fifth, and sixth classes, as well as with those still farther distant, to which he has applied it with admirable skill, and "*magno labore, multisque, nocturnis vigillis*" (as he very justly expresses himself) a better can hardly be wished for; but with stars of the second class which generally differ no more than one, two or three-tenths of a second of time in *R*, and can never differ more than four tenths, the insufficiency of measuring by time is obvious. In regard to the declination, it is also no less evident, that it is much more accurate to take an angle, which may be had true to 2 or 3° at most, than to measure its tangent, which in stars of the second class is generally no more than 2, 3, or 4'' of a degree, and can never exceed five. I do not so much as mention the stars of the first class: they must certainly, as to sense, pass the meridian at the same instant of time. Their distance has even eluded the attacks of my smallest silk-thread micrometer armed with an excellent power of 460; but I shall soon apply my last new instrument to them\*, not without hopes of success. Now, though I have hitherto not been able to express the distance of the stars of the first class, otherwise than by the proportion it bears to their apparent diameters, I think it a very great point gained, that one of my instruments at least (*viz.* the cross-hair micrometer) has laid hold of them: for their angle of position, I think, is within a very small quantity as well determined as it is in those of the second class. This simple but most useful instrument can, by actual measure,

\* For a description of which see p. 163.

discover

discover beyond a doubt a motion in two stars that are very close together, though it should amount to no more than a tenth part of a second of a degree, provided that motion be in such a direction that the effect of it be thrown upon the angle of position; wherein, with some of the stars of the first class, it would occasion an alteration of 10, 20, 30, or more degrees.

I have marked all those stars in my catalogue which have been observed by Mr. MAYER and other astronomers with an asterisk (\*) affixed to the number that they may be known; those with the mark of a dagger (+) have been observed by different astronomers before Mr. MAYER. Among the stars which are not marked, will be found several that have been observed by Mr. MAYER; but, on comparing them together, it will be seen, that they are observations of different small stars; for instance, Mr. MAYER (Act. Acad. vol. IV. p. 296.) observed a small star near Rigel at the distance of  $1' 0''$ ,  $5''$   $\mathcal{R}$  in time, and  $2' 55''$ ,  $2''$  in difference of declination north preceding Rigel. In my second class (the 34th star) we also find Rigel; but the small star I have observed is one which has not been seen by Mr. MAYER, and is at a distance of no more than  $6' 27''$ . Position  $68^{\circ} 12'$  south preceding; and so on with other stars.

I have used the expression *double-star* in a few instances of the sixth class in rather an extended signification: the example of FLAMSTEAD, however, will sufficiently authorize my application of the term. I preferred that expression to any other, such as Comes, Companion, or Satellite; because, in my opinion, it is much too soon to form any theories of small stars revolving round large ones, and therefore I thought it adviseable carefully to avoid any expression that might convey that idea. I am



very well persuaded, FLAMSTEAD, who first used the word Comes, meant it only in a figurative sense.

I shall not fail to take the first opportunity of looking out for those of Mr. MAYER's double-stars which I have not in my catalogue, amounting to 31; and also for one I find mentioned in *La Connoissance des Temps* for 1783, discovered by Mr. MESSIER.

XIII. *Description of a Lamp-Micrometer, and the Method of using it.* By Mr. William Herschel, F. R. S.

Read January 31, 1782.

THE great difficulty of measuring very small angles, such as hardly amount to a few seconds, is well known to astronomers. Since I have been engaged in observations on double stars, I have had so much occasion for micrometers that would measure exceeding small distances exactly, that I have continually been endeavouring to improve these instruments.

The natural imperfections of the parallel wire micrometer in taking the distance of very close double stars are the following. When two stars are taken between the parallels, the diameters must be included. I have in vain attempted to find lines sufficiently thin to extend them across the centers of the stars so that their thickness might be neglected. The single threads of the silk-worm, with such lenses as I use, are so much magnified that their diameter is more than that of many of the stars. Besides, if they were much less than they are, the power of deflection of light would make the attempt to measure the distance of the centers this way fruitless: for I have always found the light of the stars to play upon those lines and separate their apparent diameters into two parts. Now since the spurious diameters of the stars thus included, to my certain knowledge, are continually changing according to the state of the air, and the length of time we look at them, we are, in

some respect, left at an uncertainty, and our measures taken at different times, and with different degrees of attention, will vary on that account. Nor can we come at the true distance of the centers of any two stars, one from another, unless we could tell what to allow for the semi-diameters of the stars themselves; for different stars have different apparent diameters, which, with a power of 227, may differ from each other (as I have experienced) as far as two seconds.

The next imperfection is that which arises from a deflection of light upon the wires when they approach very near to each other; for if this be owing to a power of repulsion lodged at the surface, it is easy to understand, that such powers must interfere with each other, and give the measures larger in proportion than they would have been if the repulsive power of one wire had not been opposed by a contrary power of the other wire.

Another very considerable imperfection of these micrometers is a continual uncertainty of the real zero. I have found, that the least alteration in the situation and quantity of light will affect the zero, and that a change in the position of the wires, when the light and other circumstances remain unaltered, will also produce a difference. To obviate this difficulty, whenever I took a measure that required the utmost accuracy, my zero was always taken immediately after, while the apparatus remained in the same situation it was in when the measure was taken; but this enhances the difficulty because it introduces an additional observation.

The next imperfection, which is none of the smallest, is that every micrometer that has hitherto been in use requires either a screw or a divided bar and pinion to measure the distance of the wires or divided image. Those who are acquainted

quainted with works of this kind are but too sensible how difficult it is to have screws that shall be perfectly equal in every thread or revolution of each thread; or pinions and bars that shall be so evenly divided as perfectly to be depended upon in every leaf and tooth to perhaps the two, three, or four thousandth part of an inch; and yet, on account of the small scale of those micrometers, these quantities are of the greatest consequence; an error of a single thousandth part inducing in most instruments a mistake of several seconds.

The last and greatest imperfection of all is, that these wire micrometers require a pretty strong light in the field of view; and when I had double stars to measure, one of which was very obscure, I was obliged to be content with less light than is necessary to make the wires perfectly distinct; and several stars on this account could not be measured at all, though otherwise not too close for the micrometer.

The instrument I am going to describe, which I call a Lamp-Micrometer, is free from all these defects, and has, moreover, to recommend it, the advantage of a very enlarged scale. The construction of it is as follows.

ABGCFE (fig. 1.) is a stand nine feet high, upon which a semi-circular board *qhogp* is moveable upwards or downwards, in the manner of some fire-screens, as occasion may require, and is held in its situation by a peg *p* put into any one of the holes of the upright piece AB. This board is a segment of a circle of fourteen inches radius, and is about three inches broader than a semi-circle, to give room for the handles *rD*, *eP*, to work. The use of this board is to carry an arm *L*, thirty inches long, which is made to move upon a pivot at the center of the circle, by means of a string, which passes in a groove upon the edge of the semi-circle *pgshq*; the string is fastened to

to a hook at *o* (not expressed in the figure being at the back of the arm *L*), and passing along the groove from *ob* to *q* is turned over a pulley at *q*, and goes down to a small barrel *e*, within the plane of the circular board, where a double-jointed handle *eP* commands its motion. By this contrivance we see the arm *L* may be lifted up to any altitude from the horizontal position to the perpendicular, or be suffered to descend by its own weight below the horizontal to the reverse perpendicular situation. The weight of the handle *P* is sufficient to keep the arm in any given position; but if the motion should be too easy, a friction spring applied to the barrel will moderate it at pleasure.

In front of the arm *L* a small slider, about three inches long, is moveable in a rabbet from the end *L* towards the center backwards and forwards. A string is fastened to the left side of the little slider, and goes towards *L*, where it passes round a pulley at *m*, and returns under the arm from *m*, *n*, towards the center, where it is led in a groove on the edge of the arm, which is of a circular form, upwards to a barrel (raised above the plane of the circular board) at *r*, to which the handle *rD* is fastened. A second string is fastened to the slider, at the right side, and goes towards the center, where it passes over a pulley *n*, and the weight *w*, which is suspended by the end of this string, returns the slider towards the center when a contrary turn of the handle permits it to act.

*a* and *b* are two small lamps, two inches high,  $1\frac{1}{2}$  in breadth by  $1\frac{1}{4}$  in depth. The sides, back, and top, are made so as to permit no light to be seen, and the front consists of a thin brass sliding door. The flame in the lamp *a* is placed three-tenths of an inch from the left side, three-tenths from the front, and half an inch from the bottom. In the lamp *b* it is placed at the

the same height and distance measuring from the right side. The wick of the flame consists only of a single very thin lamp-cotton thread; for the smallest flame being sufficient it is easier to keep it burning in so confined a place. In the top of each lamp must be a little slit, lengthways, and also a small opening in one side near the upper part, to permit air enough to circulate to feed the flame. To prevent every reflection of light, the side opening of the lamp *a* should be to the right, and that of the lamp *b* to the left. In the sliding door of each lamp is made a small hole with the point of a very fine needle just opposite the place where the wicks are burning, so that when the sliders are shut down, and every thing dark, nothing shall be seen but two fine lucid points of the size of two stars of the third or fourth magnitude. The lamp *a* is placed so that its lucid point may be in the center of the circular board where it remains fixed. The lamp *b* is hung to the little slider which moves in the rabbet of the arm, so that its lucid point, in a horizontal position of the arm, may be on a level with the lucid point in the center. The moveable lamp is suspended upon a piece of brass fastened to the slider by a pin exactly behind the flame upon which it moves as a pivot. The lamp is balanced at the bottom by a leaden weight, so as always to remain upright, when the arm is either lifted above, or depressed below, the horizontal position. The double-jointed handles *rD*, *eP*, consist of light deal rods, ten feet long, and the lowest of them may have divisions, marked upon it near the end *P*, expressing exactly the distance from the central lucid point in feet, inches, and tenths.

From this construction we see, that a person at a distance of ten feet may govern the two lucid points, so as to bring them into any required position south or north preceding or following,

from 0 to 90° by using the handle P, and also to any distance from six-tenths of an inch to five or six and twenty inches by means of the handle D. If any reflection or appearance of light should be left from the top or sides of the lamps, a temporary screen, consisting of a long piece of paste-board, or a wire frame covered with black cloth, of the length of the whole arm and of any required breadth, with a slit of half an inch broad in the middle, may be affixed to the arm by four bent wires projecting an inch or two before the lamps, situated so that the moveable lucid point may pass along the opening left for that purpose.

Fig. 2. represents part of the arm L, half the real size; S the slider; *m* the pulley, over which the cord *xyz* is returned towards the center; *v* the other cord going to the pulley *n* of fig. 1. R the brass piece moveable upon the pin *c*, to keep the lamp upright. At R is a wire rivetted to the brass piece, upon which is held the lamp by a nut and screw. Fig. 3. 4. represent the lamps *a*, *b*, with the sliding doors open, to shew the situation of the wicks. W is the leaden weight with a hole *d* in it, through which the wire R of fig. 2. is to be passed when the lamp is to be fastened to the slider S. Fig. 5. represents the lamp *a* with the sliding door shut; *l* the lucid point; and *ik* the openings at the top, and *s* at the sides for the admission of air.

Every ingenious artist will soon perceive that the motions of this micrometer are capable of great improvement by the application of wheels and pinions, and other well known mechanical resources; but, as the principal object is only to be able to adjust the two lucid points to the required position and distance, and to keep them there for a few minutes, while the observer

observer goes to measure their distance, it will not be necessary to say more upon the subject.

I am now to shew the application of this instrument. It is well known to opticians and others, who have been in the habit of using optical instruments, that we can with one eye look into a microscope or telescope, and see an object much magnified, while the naked eye may see a scale upon which the magnified picture is thrown. In this manner I have generally determined the power of my telescopes; and any one who has acquired a facility of taking such observations will very seldom mistake so much as one in fifty in determining the power of an instrument, and that degree of exactness is fully sufficient for the purpose.

The Newtonian form is admirably adapted to the use of this micrometer; for the observer stands always erect, and looks in a horizontal direction, notwithstanding the telescope should be elevated to the zenith. Besides, his face being turned away from the object to which his telescope is directed, this micrometer may be placed very conveniently without causing the least obstruction to the view: therefore, when I use this instrument I put it at ten feet distance from the left eye, in a line perpendicular to the tube of the telescope, and raise the moveable board to such a height that the lucid point of the central lamp may be upon a level with the eye. The handles, lifted up, are passed through two loops fastened to the tube, just by the observer, so as to be ready for his use. I should observe, that the end of the tube is cut away so as to leave the left eye intirely free to see the whole micrometer.

Having now directed the telescope to a double star, I view it with the right eye, and at the same time with the left see it pro-



jected upon the micrometer: then, by the handle P, which commands the position of the arm, I raise or depress it so as to bring the two lucid points to a similar situation with the two stars; and, by the handle D, I approach or remove the moveable lucid point to the same distance of the two stars, so that the two lucid points may be exactly covered by, or coincide with the stars. A little practice in this business soon makes it easy, especially to one who has already been used to look with both eyes open.

What remains to be done is very simple. With a proper rule, divided into inches and fortieth parts, I take the distance of the lucid points, which may be done to the greatest nicety, because, as I observed before, the little holes are made with the point of a very fine needle. The measure thus obtained is the tangent of the magnified angle under which the stars are seen to a radius of ten feet; therefore, the angle being found and divided by the power of the telescope gives the real angular distance of the centers of a double star.

For instance, September 25, 1781, I measured  $\alpha$  Herculis with this instrument. Having caused the two lucid points to coincide exactly with the stars center upon center, I found the radius or distance of the central lamp from the eye 10 feet 4,15 inches; the tangent or distance of the two lucid points 50,6 fortieth parts of an inch; this gives the magnified angle  $35'$ ; and dividing by the power 460, which I used, we obtain  $4''\ 34'''$  for the distance of the centers of the two stars. The scale of the micrometer at this very convenient distance, with the power of 460 (which my telescope bears so well upon the fixed stars that for near a twelve-month past I have hardly used any other) is above a quarter of an inch to a second; and by putting on my power of 932, which in very fine evenings is  
1
extremely

extremely distinct, I obtain a scale of more than half an inch to a second, without increasing the distance of the micrometer; whereas the most perfect of my former micrometers, with the same instrument, had a scale of less than the two thousandth part of an inch to a second.

The measures of this micrometer are not confined to double stars only, but may be applied to any other objects that require the utmost accuracy, such as the diameters of the planets or their satellites, the mountains of the moon, the diameters of the fixed stars, &c.

For instance, October 22, 1781, I measured the apparent diameter of  $\alpha$  Lyræ; and judging it of the greatest importance to increase my scale as much as convenient, I placed the micrometer at the greatest convenient distance, and (with some trouble, for want of longer handles, which might easily be added) took the diameter of this star by removing the two lucid points to such a distance as just to inclose the apparent diameter. When I measured my radius it was found to be twenty-two feet six inches. The distance of the two lucid points was *about* three inches; for I will not pretend to *extreme* nicety in this observation, on account of the very great power I used, which was 6450. From these measures we have the magnified angle  $38' 10''$ : this divided by the power gives  $0''.355$  for the apparent diameter of  $\alpha$  Lyræ. The scale of the micrometer, on this occasion, was no less than 8,443 inches to a second, as will be found by multiplying the natural tangent of a second with the power and radius in inches.

November 28, 1781, I measured the diameter of the new star; but the air was not very favourable, for this singular star was not so distinct with 227 that evening as it generally is

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with 460: therefore, without laying much stress upon the exactness of the observation, I shall only report it to exemplify the use of the micrometer. My radius was 35 feet 11 inches. The diameter of the star, by the distance of the lucid points, was 2.4 inches, and the power I used 227: hence the magnified angle is found  $19'$ , and the real diameter of the star  $5''.022$ . The scale of this measure, 474 millesimals of an inch, or almost half an inch to a second.

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**XIV.** *A Paper to obviate some Doubts concerning the great Magnifying Powers used. By Mr. Herschel, F. R. S.*

TO SIR JOSEPH BANKS, BART. F. R. S.

SIR,

I HAVE the honour of laying before you the result of a set of measures I have taken in order to ascertain once more the powers of my Newtonian seven-feet reflector. The method I have formerly used, and which I still prefer to that which I have now been obliged to practise, requires very fine weather and a strong sun-shiny day; but my impatience to answer the requests of Sir JOSEPH BANKS would not permit me to wait for so precarious an opportunity at this season of the year. The difference in all the powers, as far as 2010, will be found to be in favour of those I have mentioned; and, I believe, a much greater concurrence could not well be expected, where  
different

different methods of ascertaining them are used. The variation in the two highest powers is more considerable than I was aware of; but still may easily be shewn to be a necessary consequence of the difference in the methods. However, if upon comparing together the methods it should be thought, that the power 5786 is nearer the truth than 6450, I shall readily join to correct that number. The manner in which I have now determined the powers is as follows: I took one of the eye lenses which magnifies least, and measured its solar focus by the sun's rays as exactly as I could five times, which proved to be 1.01, 1.04, 1.09, 1.01, 1.05, in half-inch measure, a mean of which is 1.04. The sidereal focus of my seven-feet speculum is 170.4 in the same measure. Thence, dividing 170.4 by 1.04 we find that the telescope will magnify 163.8 times when that lens is used. This power being found, I applied the same lens as a single microscope to view with it a certain object, which was a drawn brass wire fastened so as not to turn upon its axis or change its position; for these wires are seldom perfectly round, or of an even size, and it is therefore necessary to use this precaution to prevent errors: then, with a fine pair of compasses, I took four independent measures of the image of the brass wire, which was thrown upon a sheet of paper exactly  $8\frac{1}{4}$  inches from the lens, the eye being always as close to the lens as possible. I viewed the same wire, exactly in the same manner, with every one of the lenses, and measured the pictures upon the paper. When I came to the higher powers the wire was exchanged for another 4.37 times thinner than the former, as determined by comparing the proportion of their images 54 to  $235\frac{1}{4}$ , taken by the same lens.

When the images of these wires are obtained, the power of the telescope, with every one of the lenses, becomes known by

by one plain analogy : viz. as the image of the wire by the first lens (77½) is to the power it gives to the telescope (163.8), so is the image of the wire by the second lens (119) to the power it will give to the same telescope (250.7). The particulars of all the measures are as follows :

Powers as they have been called in my papers.	Images of a wire thrown upon a paper in hundredths of half inches.				A mean of the four measures.	Powers as they come out by this method.
146	77	78	78	78	77½	163.86 = $\frac{170.4}{1.04}$
227	119	119	119	119	119	250.7
278	143	143	144	143	143½	301.8
460	236	236	235	236	235½	496.7
	Smaller wire.				54	
	53	54	55	54	54	
754	83	85	84	85	84½	775.1
934	107	107	107	108	107½	986.7
1159	128	128	129	128	128½	1179.9
1536	An excellent lens, lost about eight months ago.					
2010	236	236	238	236	236½	2175.8
3168	281	283	281	280	281½	2585.5
6450	635	625	630	626	629	5786.8

I beg leave, Sir, now to give a short description of the method I have formerly used to determine these powers. In the year 1776 I erected a mark of white paper, exactly half an inch in diameter, which I viewed with my telescopes at the greatest convenient distance with one of the least magnifiers. An assistant was placed at rectangles in a field, at the same distance from



from my eye as the object from the great speculum of the telescope. Upon a pole erected there I viewed the magnified image of the half inch, and the assistant marked it by my direction; this being measured gave the power of the instrument at once. The power thus obtained was corrected by theory, to reduce it to what it would be upon infinitely distant objects. The powers of the rest of the lenses I deduced from this by a *Camera-eye-piece*, which I made for that purpose. ABCD (fig. 1.) represents a perpendicular section of it. The end A screws into the telescope. Upon the end B may be screwed any of the common single-lens eye-pieces. *lmn* is a small oval plane speculum, adjusted to an angle of  $45^\circ$  by three screws, two whereof appear at *op*. When the observer looks in at B, he may see the object projected upon a sheet of paper on a table placed under the Camera-piece, and measure its picture *a, b*, as in fig. 2. The power of one lens therefore being known, that of the rest was also found by comparing the measures of the projected images.

It may not be amiss to mention some of the advantages and inconveniencies attending each of these methods. When we take the focus of an eye-lens, which the first method requires, we are liable to a pretty considerable uncertainty, and in very small lenses it is not to be done at all. Moreover, in calculating the power by that focus no account is made of the aberration which takes place in all specula and lenses, and increases the image, so that we rather find out how much the telescope *should* magnify than how much it really *does* magnify; but in determining the power by an experiment we avoid these difficulties.

On the other hand, when the power is very great, the latter method becomes inconvenient, both on account of want of light

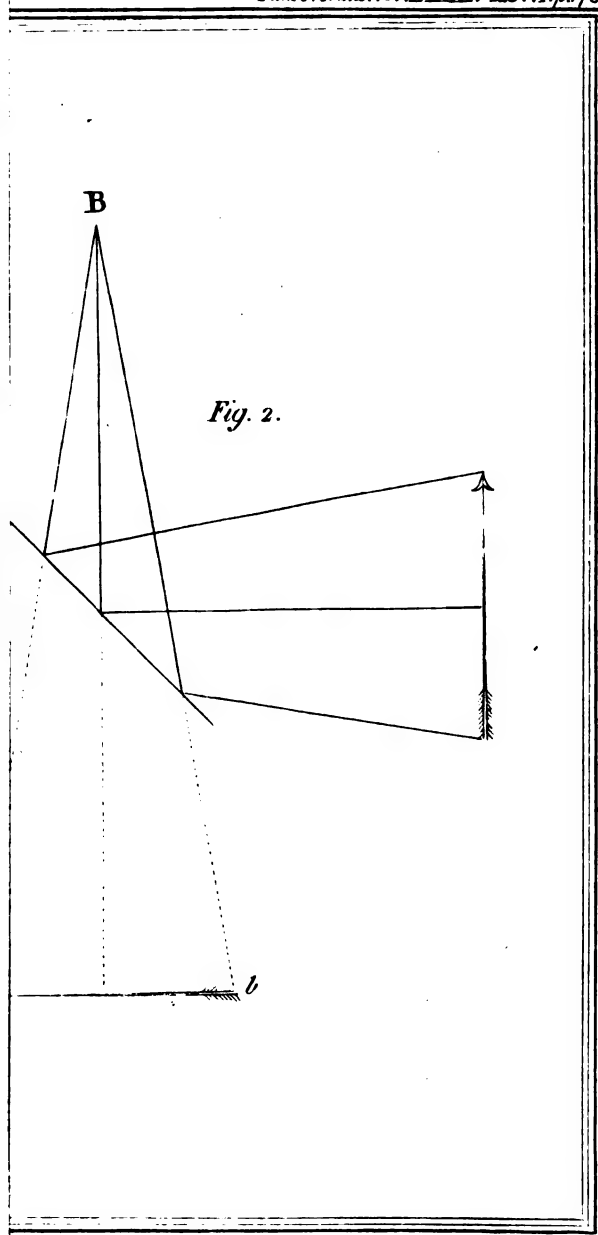
light in the object, and a very considerable aberration which takes place, and makes the picture too indistinct to be very accurate in the measure, and of course larger than it ought to be; and this will account for the excess in the measures of my two largest powers. However, when I employed 6450 upon the diameter of  $\alpha$  Lyræ, I incline to think the method I had used when I determined that power, ought to be preferred, because my Lamp-micrometer gives the measure of an object as it appears in the telescope, and therefore this aberration is included, and should be taken into consideration.

To prevent any mistakes, I wish to mention again, that I have all along proceeded *experimentally* in the use of my powers, and that I do not mean to say I have used 6450 (or 5786) upon the planets, or even upon double stars; every power I have mentioned is to be understood as having been used just as it is related; but farther inferences ought not as yet to be drawn. For instance, my observations on  $\alpha$  Bootis mention that I have viewed that star with 2010 (or as in the above table with 2175) extremely distinct; but upon several other celestial objects I have found this power of no service. Many plausible suggestions have already occurred to account for these appearances; but I wait till farther experiments shall have furnished me with more materials to reason upon. The use of high powers is a new and untrodden path, and in this attempt variety of new phænomena may be expected, therefore I wish not to be in a haste to make general conclusions. I shall not fail to pursue this subject, and hope soon to be able to attack the celestial bodies with a still stronger armament, which is now preparing.

It remains now only for me to make the most sincere acknowledgement for the favours you have shewn to me, and to say that I shall ever remain, with equal respect and gratitude,

S I R, your most obedient, &c.

P. S. Dr. WATSON junior has done me the favour separately to examine and measure the powers of my telescope; and placing the greatest confidence in his accuracy, I rely on his measures at least as much as my own.





XIV. *Continuation of the Experiments and Observations on the Specific Gravities and Attractive Powers of various Saline Substances.* By Richard Kirwan, Esq. F. R. S.

Read April 11, 1782.

**B**EFORE I enter into a detail of the new experiments I have made in the prosecution of this subject, I must beg leave to rectify some mistakes I have fallen into in my last paper.

1. In computing the quantity of acid taken up by 10,5 gr. of mild vegetable fixed alkali, I made no allowance for the small quantity of earth it contains, viz. 0,7035 of a grain; but in large quantities of alkali, this proportion is considerable, and occasioned a small but sensible error in my subsequent calculations of the proportion of ingredients in neutral salts, the quantity of alkali being, by that fraction, less than I supposed it in 10,5 gr. This correction being made, it will be found, that 100 gr. of perfectly dry vegetable fixed alkali (abstracted from the quantity of earth) generally contain 22,457 gr. of fixed air instead of 21, as I before determined; yet the former determination is right, where the earth is not separated, yet may well be supposed to exist, as in the alkali of pearl-ash, purified by three repeated calcinations and solutions. Hence also 100 gr. of such alkali, free from earth, water, and fixed air, take up 46,77 gr. of the mineral acids, that is, of the mere

A a 2

acid

acid part; and 100 gr. of common mild vegetable alkali take up about 36,23 of real acid.

100 gr. of perfectly dry tartar vitriolate contain 30,21 of real acid, 64,61 of fixed alkali, and 5,18 of water. Crystallized tartar vitriolate loses only 1 *per cent.* of water in a heat in which its acid also is not separated in any degree, and therefore contains 6,18 of water.

100 gr. of nitre, perfectly dried, contain 30,86 of acid, 66 of alkali, and 3,14 of water; but in crystallized nitre the proportion of water is somewhat greater; for 100 gr. of these crystals, being exposed to a heat of  $180^{\circ}$  for two hours, lost 3 gr. of their weight, without exhaling any acid smell; but when exposed to a heat of  $200^{\circ}$ , the smell of the nitrous acid is distinctly perceived. Hence 100 gr. of crystallized nitre contain 29,89 of mere acid, 63,97 of alkali, and 6,14 of water;

100 gr. of digestive salt perfectly dry contain 29,68 of marine acid, 63,47 of alkali, and 6,85 of water. 100 gr. of crystallized digestive salt lose but 1 gr. of their weight before the smell of the marine acid is perceived; and hence they contain 7,85 gr. of water.

But the mistake which cost me most time and pains to correct was that which I fell into when I imagined, that the mixtures of oil of vitriol and water, and spirit of nitre and water, had attained their maximum of density when they had cooled to the temperature of the atmosphere, which at the time I made my experiments stood between  $50$  and  $60^{\circ}$  of FAHRENHEIT. The former I had even suffered to stand six hours, which was much longer than was necessary for its cooling; but when the acid was so much diluted as to cause little or no heat, I allowed it to stand but for a very little time before I examined its density: yet several months after I found many of these mixtures

mixtures much denser than when I first examined them, and that at least twelve hours rest was requisite before concentrated oil of vitriol, to which even twice its weight of water is added, attains its utmost density, and still more when a lesser proportion of water is used: thus, when I made the mixture of 2519.75 gr. of oil of vitriol, whose specific gravity was 1.819, with 180 of water, I found its density six hours after 2.771; but after twenty-four hours it was 1.798: and hence, according to the reasoning in the former paper, the accrued density was at least .064 instead of .045 as I had formerly found it. But by using oil of vitriol still more concentrated, whose specific gravity was 1.8846, I was enabled, by a similar train of reasoning, to make a still nearer approximation, and found that the accrued density of oil of vitriol, whose specific gravity is 1.819, amounts to 0.104; and consequently its mathematical specific gravity is 1.715. 6.5 gr. of this oil of vitriol contained, as I before found, 3.55 of mere acid, and the remainder water, then the weight of an equal bulk of water is 3.79 gr.: and subtracting from this the weight of the water that enters into the composition of the oil of vitriol, it will be found, that the weight of a bulk of water, equal to the acid part, is 0.84, and consequently the specific gravity of the pure and mere acid part is 4.226. Upon this ground, and constantly allowing the mixtures to rest at least twelve hours (until the oil of vitriol was diluted with four times its weight of water, and then often only six hours) before their density was examined, I constructed the table hereto annexed; the temperature of the room I constantly kept between 50 and 60°.

Oil



Oil of vitriol.	Acid.	Water.	Accrued density.	Mathemat. sp. gravity.	
Grains.					
1000	- -	387.95	,07	1,877	1,846
1100	- -	487.95	,104	1,738	1,844
1200	- -	587.95	,105	1,637	1,742
1300	- -	687.95	,144	1,561	1,705
1400	- -	787.95	,144	1,500	1,644
1500	- -	887.95	,137	1,452	1,589
1600	- -	987.95	,137	1,412	1,539
1700	- -	1087.95	,130	1,379	1,509
1800	- -	1187.95	,124	1,350	1,474
1900	- -	1287.95	,116	1,326	1,442
2000	- -	1387.95	,116	1,304	1,420
2100	- -	1487.95	,112	1,286	1,398
2200	- -	1587.95	,112	1,269	1,381
2300	- -	1687.95	,108	1,254	1,362
2400	- -	1787.95	,104	1,241	1,345
2500	- -	1887.95	,104	1,229	1,333
2600	- -	1987.95	,101	1,219	1,320
2700	- -	2087.95	,096	1,209	1,307
2800	- -	2187.95	,091	1,200	1,291
2900	- -	2287.95	,090	1,192	1,282
3000	- -	2387.95	,090	1,184	1,274
3100	612,05	2487.95	,090	1,177	1,267
3200	- -	2587.95	,090	1,170	1,260
3300	- -	2687.95	,089	1,164	1,253
3400	- -	2787.95	,084	1,159	1,243
3500	- -	2887.95	,083	1,150	1,233
3600	- -	2987.95	,073	1,149	1,222
3700	- -	3087.95	,073	1,144	1,217
3800	- -	3187.95	,071	1,140	1,211
3900	- -	3287.95	,071	1,136	1,208
4000	- -	3387.95	,071	1,132	1,204
4100	- -	3487.95	,070	2,128	1,198
4200	- -	3587.95	,070	1,125	1,195
4300	- -	3687.95	,070	1,121	1,191
4400	- -	3787.95	,070	1,118	1,188
4500	- -	3887.95	,070	1,115	1,185
4600	- -	3987.95	,070	1,113	1,183
4700	- -	4087.95	,070	1,110	1,180
4800	- -	4187.95	,070	1,107	1,177
4900	- -	4287.95	,070	1,105	1,175
5000	- -	4387.95	,070	1,103	1,172
5100	- -	4487.95	,069	1,100	1,169

Oil

Oil of vitriol.	Acid.	Water.	Accrued density	Mathemat. sp. gravity.	
Grains.					
5200	- -	4587,95	,069	1,098	1,167
5300	- -	4687,95	,069	1,096	1,165
5400	- -	4787,95	,069	1,094	1,163
5500	- -	4887,95	,068	1,092	1,160
5600	- -	4987,95	,067	1,091	1,158
5700	- -	5087,95	,067	1,089	1,156
5800	- -	5187,95	,067	1,087	1,154
5900	- -	5287,95	,065	1,086	1,151
6000	- -	5387,95	,064	1,084	1,148
6100	612,05	5487,95	,064	1,082	1,146
6200	- -	5587,95	,063	1,081	1,144
6300	- -	5687,95	,062	1,080	1,142
6400	- -	5787,95	,062	1,078	1,140
6500	- -	5887,95	,061	1,077	1,138
6600	- -	5987,95	,060	1,076	1,136
6700	- -	6087,95	,060	1,074	1,134
6800	- -	6187,95	,060	1,072	1,132
6900	- -	6287,95	,060	1,070	1,130
7000	- -	6387,95	,059	1,069	1,128

With regard to the nitrous acid, I found also I had been a little too precipitate as to the time of examining its density after it had been mixed with water. Hence, making use of some whose specific gravity was 1,474, I allowed the mixtures to rest twelve hours, until it was diluted with twice its weight of water, and the subsequent mixtures six hours at least; by the former process of reasoning, I found the specific gravity of the mere nitrous acid to be 5,530.

Spirit

Spirit of nitre.	Acid.	Water.	Accrued density.	Mathemat. sp. gravity.	Physical sp. gravity.
900	-	507	-	1,557	1,557
1000	-	607	-	1,474	1,474
1100	-	707	,035	1,413	1,448
1200	-	807	,056	1,367	1,423
1300	-	907	,065	1,329	1,394
1400	-	1007	,065	1,298	1,363
1500	-	1107	,077	1,273	1,350
1600	-	1207	,082	1,251	1,333
1700	-	1307	,082	1,233	1,315
1800	-	1407	,083	1,217	1,300
1900	-	1507	,083	1,204	1,287
2000	-	1607	,096	1,191	1,269
2100	-	1707	,088	1,181	1,254
2200	-	1807	,071	1,176	1,247
2300	-	1907	,068	1,162	1,230
2400	-	2007	,068	1,154	1,222
2500	-	2107	,067	1,147	1,214
2600	-	2207	,065	1,141	1,206
2700	-	2307	,063	1,135	1,198
2800	-	2407	,061	1,129	1,190
2900	-	2507	,058	1,124	1,182
3000	-	2607	,055	1,120	1,175
3100	393	2707	,054	1,116	1,170
3200	-	2807	,054	1,111	1,165
3300	-	2907	,053	1,108	1,161
3400	-	3007	,052	1,104	1,156
3500	-	3107	,050	1,101	1,151
3600	-	3207	,048	1,098	1,146
3700	-	3307	,047	1,095	1,142
3800	-	3407	,045	1,092 or 3	1,137
3900	-	3507	,043	1,089	1,132
4000	-	3607	,040	1,087	1,127
4100	-	3707	,037	1,085	1,122
4200	-	3807	,035	1,083	1,118
4300	-	3907	,034	1,080	1,114
4400	-	4007	,032	1,078	1,110
4500	-	4107	,029	1,077	1,106
4600	-	4207	,027	1,075	1,102
4700	-	4307	,025	1,073	1,098
4800	-	4407	,022	1,072	1,094
4900	-	4507	,020	1,070	1,090
5000	-	4607	,018	1,068	1,086
5100	-	4707	,015	1,067	1,082
5200	-	4807	,012	1,066	1,078
5300	-	4907	,008	1,066	1,074

∴ The foregoing experiments were made at the temperature of between 50 and 60° of FAHRENHEIT ; but as it may be suspected, that the density of the above acids is considerably altered at degrees of temperature considerably different, I endeavoured to find the quantity of this alteration, and to calculate what this density would be at 55°, that the quantities of acid and water may thereby be investigated.

To this end I took some dephlogisticated spirit of nitre, and examined its specific gravity at different degrees of heat, and found it as follows :

	Deg.		Sp. gravity.
at {	30	-	1,4653
	46	-	1,4587
	86	-	1,4302
	120	-	1,4123

Therefore, the total expansion of this spirit of nitre from 30 to 120°, that is, by 90° of heat, was 0,0527 ; for  $1,4650 - 1,4123 = ,0527$ , by which we see that the dilatations are nearly proportional to the degrees of heat : for beginning with the first dilatation from 30 to 46°, that is, by 16° of heat  $\div 90 \cdot 0,0527 :: 16 \cdot 0,0093$  ; but in reality these 16° of heat afforded a dilatation equal only to 0,0063 ; for  $1,4650 - 1,4587 = 0,0063$  ; so that the difference betwixt the calculated and observed dilatations is only  $\frac{1}{16000}$ , a difference of no consequence in the present case, and that might arise from the immersion of the cold glass ball filled with mercury in the liquor, it being the solid I use to try the specific gravity of liquids. In the next case the difference is still less ; for  $\div 90 \cdot 0,0527 :: 56 \cdot 0,0327$  ; but 56° of heat produced in reality a dilatation of 0,0348 for  $1,4650 - 1,4302 = 0,0348$ , so that the calculation is deficient only in  $\frac{1}{16000}$ .

I afterwards tried another, and somewhat stronger, spirit of nitre, whose specific gravity was,

	Deg.		Sp. gravity.
at {	34	-	1,4750
	49	-	1,463
	150	-	1,3792

Here also the expansions are nearly proportional to the degrees of heat; for  $116^{\circ}$  of heat (the difference between 34 and 150) produce an expansion of 0,0958; and  $15^{\circ}$  of heat (the difference between 34 and 49) produce an expansion of 0,0097, and by calculation 0,0123, which last differs from the truth only by  $\frac{26}{100000}$ .

By this experiment we see, that the stronger the spirit of nitre is, the more it is expanded by the same degree of heat: for if the spirit of nitre of the last experiment were expanded in the same proportion as in the first, its dilatation by  $116^{\circ}$  of heat should be 0,0679, whereas it was found to be 0,0958.

As the dilatation of spirit of nitre is far greater than that of water by the same degree of heat, and as it consists only of acid and water, it clearly follows, that its superior dilatability must be owing to the acid part; and hence, the more acid is contained in a given quantity of spirit of nitre, the greater is its dilatability. We might therefore suppose, that the dilatation of spirit of nitre was intermediate betwixt that of the quantity of water it contains and that of its quantity of acid; but there exists another power also which prevents this simple result, namely, the mutual attraction of the acid and water to each other, which makes them occupy a less space than the sum of their joint volumes, which condensation I have therefore called their *accrued density*. Taking this into the account,

we may consider the dilatation of spirit of nitre as equal to those of the quantities of water and acid it contains minus the condensation they acquire from their mutual attraction, and this rule holds as to all other heterogeneous compounds.

To find the quantities of acid and water in spirit of nitre, whose specific gravity was found in degrees of temperature different from those for which the table was constructed, viz. 54, 55, or 56° of FAHRENHEIT, the surest method is to find how much that spirit of nitre is expanded or condensed by a greater or lesser degree of heat, and then, by the rule of proportion, find what its density would be at 55°; but if this cannot be done, we shall approach pretty near the truth, if we allow  $\frac{1}{1000}$  for every 15° of heat above or below 55° of FAHRENHEIT, when the specific gravity of spirit is between 1,400 and 1,500; and  $\frac{1}{1000}$  when the specific gravity is between 1,400 and 1,300.

As to oil and spirit of vitriol I found the dilatations exceeding irregular, probably by reason of a white foreign matter, which is more or less suspended or dissolved in it, according to its greater or lesser dilution. This matter I would not separate, as I intended trying the density of this substance in the state in which it is commonly used. In general I found, that 15° of heat cause a difference of about  $\frac{1}{1000}$  in its specific gravity when it exceeds 1,800; and of  $\frac{1}{1000}$  when its specific gravity is between 1,400 and 1,300, its dilatation is greater than that of water, and so much greater as it is stronger.

The dilatations of spirit of salt are very nearly proportional to the degrees of heat, as appears by the following table.

	Deg.		Sp. gravity.
at	33	-	1,1916
	54	-	1,1860
	66	-	1,1820
	128	-	1,1631

Hence  $\frac{1}{1000}$  should be added, or subtracted for every  $21^{\circ}$  above or below  $55^{\circ}$  in order to reduce it to  $55^{\circ}$ , the degree for which its proportion of acid and water was calculated. The dilatibility of this acid is much greater than that of water, and even than that of the nitrous acid of the same density.

I now proceed to examine the quantity of pure acids taken up at the point of saturation by the various substances they unite with.

## OF THE MINERAL ALKALI.

That which I made use of was procured from Mr. TURNER, who by a peculiar and ingenious process extracts it in the greatest purity form common salt.

Of this alkali I rendered a portion tolerably caustic in the usual manner, and evaporating 1 oz. of the caustic solution to perfect dryness, I found it to contain 20,25 gr. of solid matter. I was assured, that the watery part alone exhaled during the evaporation, as the quantity of fixed air contained in it was very small, and to dissipate this a much greater heat would be requisite than that which I used. This dry alkali I immediately dissolved in twice its weight of water, and saturating it with dilute vitriolic acid, found it to contain 2,25 gr. of fixed air, that being the weight which the saturated solution

solution wanted of being equal to the joint weights of the water, alkali, and spirit of vitriol employed.

The quantity of mere vitriolic acid necessary to saturate 100 gr. of pure mineral alkali I found to be 60 or 61 gr. the saturated solution, thus formed, being evaporated to perfect dryness weighed 36,5 gr. ; but of this weight only 28,38 were alkali and acid, therefore the remainder, that is, 8,12 gr. were water. Hence, 100 gr. of GLAUBER'S salt, perfectly dried, contain 29,12 of mere vitriolic acid, 48,6 of mere alkali, and 22,28 of water ; but GLAUBER'S salt crystallized contains a much larger proportion of water ; for 100 gr. of these crystals being heated red-hot lost 55 gr. of their weight. This loss I suppose to arise merely from the evaporation of the watery part, and the remaining 45 contained alkali, water, and acid, in the same proportion as the 100 gr. of GLAUBER'S salt, perfectly dried, abovementioned ; then these 45 contained 13,19 gr. of vitriolic acid, 21,87 of fixed alkali, and 9,94 of water ; consequently 100 gr. of crystallized GLAUBER'S salt contain 13,19 of vitriolic acid, 21,87 of alkali, and 64,94 of water.

I also saturated this alkali with the dephlogistified nitrous acid, and found that 100 gr. of the alkali took up 57 of the mere nitrous acid in the experiment I most depended on ; but this quantity varied in some experiments a few grains, being sometimes 60, and sometimes 63 gr. ; so that I conclude the proportion of this acid, taken up by the alkali, is nearly the same as that of the vitriolic acid. Supposing this quantity to be 57 gr. then 100 gr. Cubic nitre, perfectly dry, contain 30 of acid, 52,18 of alkali, and 17,82 of water ; but Cubic nitre crystallized contains something more water ; for 100 gr. of these crystals lose about 4 by gentle drying ; therefore 100 gr. of  
of



of the crystallized salt contain 28,8 of acid, 50,09 of alkali, and 21,11 of water.

Of mere marine acid, 100 gr. of this alkali required from 63 to 66 or 67 gr.; perhaps one reason of this variety is, that it is exceeding hard to hit the true point of saturation. Allowing it to be 66 gr. then 100 gr. of perfectly dry *common salt* contain nearly 35 of real acid, 53 of alkali, and 13 of water; but 100 gr. of the crystallized salt lose 5 by evaporation; then 100 gr. of these crystals contain 33,3 of acid, 50 of alkali, and 16,7 of water.

The proportion of fixed air, alkali, and water, in crystallized mineral alkali, I investigated thus: 200 gr. of these crystals were dissolved in 240 of water; the solution was saturated by such a quantity of spirit of nitre as contained 40 of mere nitrous acid; hence I inferred, that these 200 gr. of alkali contained 70 of real alkali. The saturate solution weighed 40 gr. less than the sum of its original weight, and that of the spirit of nitre added to it; therefore it lost 40 gr. of fixed air. The remainder, therefore, of the original weight of the crystals must have been water, that is, 90 gr.; consequently 100 gr. of these crystals contained 35 of alkali, 20 of fixed air, and 45 of water.

This proportion is, particularly with regard to the alkali, very different from that found by Mr. BERGMAN and LAVOISIER, which I impute to their having used soda recently crystallized. Mine had been made some months, and probably lost much water and fixed air by evaporation, which altered the proportion of the whole. According to the calculation of these philosophers 100 gr. of this alkali takes up 80 of fixed air.

The

The specific gravity of the crystallized mineral alkali weighed in æther I found to be 1,421.

## OF THE VOLATILE ALKALI.

It is not possible by the old chymical methods to find the proportion of the ingredients in volatile alkalies, whether in a liquid or in a concrete state; seeing that, though it may be separated from fixed air, yet it cannot from water, on account of its extreme volatility. Then to find this proportion we must recur to the experiments of Dr. PRIESTLEY, who by his new analysis produced this alkali free from the ærial acid and water in the form of air: and in the third volume of his Observations, p. 294. informs us, that 1<sup>st</sup> measures of alkaline air take up, and are saturated by, 1 measure of fixed air. Let us suppose the measure to contain 100 cubic inches; then 185 cubic inches of alkaline air take up 100 of fixed air; but 185 cubic inches of alkaline air weigh, at a medium, 42,55 gr.; and 100 cubic inches of fixed air weigh 57 gr.; then 100 gr. of pure volatile alkali, free from water, take up 134 of fixed air.

On expelling its ærial acid from a parcel of this alkali in a concrete state, and formed by sublimation, I found 100 gr. of it to contain 53 of fixed air, and therefore, according to the preceding reasoning, 39,47 of real alkali and 7,53 of water *per cent*.

Saturating a solution of this alkali with the vitriolic, nitrous, and marine acids, I found, that 100 gr. of the mere alkali take up 106 of mere vitriolic acid, 115 of the nitrous, and 30 of the marine.

The

The specific gravity of the concrete volatile alkali weighed in æther was 1,4076.

The proportion of water in the different ammoniacal salts I have not been able to find, on account of their volatility; but believe it to be very small, as volatile alkali and fixed air crystallize without the help of water, when both are in an aerial state.

## OF CALCAREOUS EARTH.

I first dissolved this earth in the nitrous acid, and found that, after allowing for the loss of fixed air and the quantity of water I formerly mentioned, 100 gr. of the pure earth take up 104 of mere nitrous acid. Instead of dissolving this earth immediately in the vitriolic acid, I precipitated its solution in the nitrous by the gradual addition of the vitriolic, and found that to effect this 91 or 92 gr. only of mere vitriolic acid were required.

100 gr. of this pure earth demand for their solution 112 of mere marine acid. The solution, which is at first colourless, grows greenish on standing. Natural Gypsum varies in its proportion of acid, earth, and water, 100 gr. of it containing from 32 to 34 of acid, and also of earth, and from 26 to 32 of water. The artificial contains 32 of earth, 29,44 of acid, and 38,56 of water; when well dried it loses about 24 of water, and therefore contains 42 of earth, 39 of acid, and 19 of water *per cent.*

100 gr. *nitrous selenite*, carefully dried, contain 33,28 of acid, 32 of earth, and 34,72 of water.

100 gr. *marine selenite*, well dried, so as to lose no part of the acid, contain 42,56 of acid, 38 of earth, and 19,44 of water.

OF

## OF MAGNESIA OR MURIATIC EARTH.

This earth, perfectly dry and free from fixed air, could not be dissolved in any of the acids without heat. In the temperature of the atmosphere even the strongest nitrous acid did not act upon it in twenty-four hours; but in a heat of  $180^{\circ}$  these acids diluted with four or six times their quantity of water attacked it very sensibly; but as much of the acids is dissipated by heat, I could not judge of the exact quantity of acid requisite to dissolve a given quantity of it, any other-wise than by precipitating the solutions by another substance, whose capacity for taking up acids was known. The substance I used was a tolerably caustic vegetable alkali. By this method I found, that 100 gr. of pure magnesia take up 125 gr. of mere vitriolic acid, 132 of the nitrous, and 140 of the marine. None of these solutions reddened vegetable blues; all of them appeared to contain something gelatinous; that in the marine acid became greenish on standing for some time.

100 gr. of perfectly dry Epsom salt contain 45,67 of mere vitriolic acid, 36,54 of pure earth, and 17,83 of water; but 100 gr. of crystallized Epsom lose 48 by drying, and consequently contain 23,75 of acid, 19 of earth, and 57,25 of water. Common Epsom salt contains an excess of acid, for its solution reddens vegetable blues.

100 gr. of nitrous Epsom, well dried, contain 35,64 of acid, 27 of pure earth, and 37,36 of water.

The solution of marine Epsom cannot be tolerably dried without losing much of its acid; together with the water.

The specific gravity of pure muriatic earth is 2,3296.

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OF

## OF EARTH OF ALLUM OR ARGILLACEOUS EARTH.

This earth I found to contain about 26 *per cent.* of fixed air, though I had previously kept it red-hot for half an hour : this surprised me much, as most writers say it contains scarce any. It dissolved in acids with a moderate effervescence until the heat was raised to  $220^{\circ}$ , after which I found the solution lighter than the quantities employed in the proportion I mentioned.

100 gr. of this earth (exclusive of the fixed air) require 133 of the *mere* vitriolic acid to dissolve them. This solution I made in a very dilute spirit of vitriol, whose specific gravity was 1,093, in which the proportion of acid to that of water was nearly as 1 to 14. This solution contained a slight excess of acid, turning vegetable blues into a brownish red; but it crystallized when cold, and the crystals were of the form of allum; so that I believe this to be nearly the proper proportion of its acid and earth; but there was not water enough to form large crystals. As this solution contained an excess of acid, I added more earth to it, but could not prevent its tinging blue paper red, until it formed an insoluble salt, that is, one that required an exceeding large quantity of water to dissolve it, and while part was thus become insoluble, yet another part would still retain an excess of acid; so that at the same time part would be supersaturated with earth, and another with acid, if tinging vegetable blues be a mark of an excess of acidity, which indeed in this case seems dubious.

100 gr. of Allum, perfectly dried, contain 42,74 of acid, 32,14 of earth, and 25,02 of water; but crystallized allum loses 44 *per cent* by desiccation; therefore 100 gr. of it contain 23,94 acid, 18 of earth, and 58,06 of water.

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100 gr. of this pure earth take up as far as I can judge 153 of the mere nitrous acid. The solution still reddened vegetable blues; but after the addition of this quantity of pure earth, I think it was, that an insoluble salt came to be formed. The solution, when cold, grew turbid, and could not be wholly dissolved by 500 times its weight of water.

The same quantity of pure earth requires 173.45 of the mere marine acid for its solution; but the solution still reddens vegetable blues. After this an insoluble salt was formed; but the beginning of its formation is difficultly discovered both in this and in the former cases.

The specific gravity of argillaceous earth, containing 25 per cent. of fixed air, I found to be 1,9901.

## OF PHLOGISTON.

Before I proceed to investigate its proportion in various compounds, and particularly in phlogificated acids, it will be necessary to say something of its nature.

It is allowed on all hands, that fixed air, or the Aërial Acid, as it is more properly called, is capable of existing in two states; the one fixed, concrete, and unelastic, as when it is actually combined with calcareous earth, alkalies, or magnesia; the other, fluid, elastic, and aëriform, as when it is actually disengaged from all combination. In its concrete and unelastic state it can never be produced single and disengaged from other substances; for the moment it is separated from them, it assumes its aërial and elastic form. The same thing may be said of phlogiston: it can never be produced in a *concrete state*, single and uncombined with other substances; for the instant it

is disengaged from them, it appears in a fluid and elastic form, and is then commonly called *inflammable air*. These different states of the same substance arise, according to the immortal discoveries of Dr. BLACK, from the different portions of elementary fire contained in such substance, and absorbed by it, whilst its sensible heat remains the same, and hence called its *specific fire*. For want of attention to these different states, the very existence of phlogiston as a distinct principle has been frequently called in question, and chemists have been required to exhibit it separate in its fixed state, without recollecting, that neither can fixed air be shewn separate in a concrete state, nor that phlogiston may also be in the same predicament; while others have totally mistaken the nature of inflammable air, and imagined it to be a combination of acid and phlogiston. The reason why fixed air cannot be separated from any substance in a concrete state is, because when it is separated, for instance by means of an acid, there is always a double decomposition, the acid yielding its specific quantity of fire to the concrete fixed air, which then assumes an ærial form, while the fixed air yields the substance it was combined with to the acid. This is so true, that though a solution of lime in the nitrous acid yields a considerable quantity of heat, yet a solution of chalk in that acid scarcely yields any; for all the fire that is set loose, and rendered sensible in the first case, is absorbed by the fixed air in the second case, being precisely that which converts it into an ærial form. The separation of phlogiston from a metallic earth in the form of inflammable air arises from the same cause, the dissolving acid yielding its fire to the phlogiston, which then assumes an ærial form, while the phlogiston yields the metallic earth to the acid. It is true, that much sensible heat is produced on this occasion, for which

three

three substantial reasons may be assigned; first, the proportion of fixed air in a given weight of crude calcareous earth, is much greater than that of phlogiston in any metal, as will hereafter be shewn, it being in the former one-third of the whole, and that of phlogiston in the latter for the most part not even one-sixth. Secondly, much of the phlogiston combines with the acid itself during the solution, and expels part of its specific quantity of fire, as Dr. CRAWFORD has shewn, and as I have since experienced; and this fire must occasion sensible heat. Thirdly, much of the phlogiston, during solution, unites to the surrounding atmosphere, expelling also part of its specific fire, and this also must occasion sensible heat; and hence it is, that metallic solutions *in vacuo* are generally attended with less heat, though with a more violent effervescence than in open air. The solution of metallic calces is not attended with as much heat as that of their respective metals, not only because neither the dissolving acids nor the surrounding air is much phlogisticated; but also because they contain an elastic fluid in a concrete state, which absorbs much of the fire given out by the dissolving acids, as it acquires an aerial state.

The origin and formation of inflammable air being thus explained, I now proceed to shew its identity and homogeneity with phlogiston. By phlogiston is generally understood that principle in combustible bodies on which their inflammability principally depends; that principle to which metals owe their malleability and splendor; that which combined with vitriolic acid forms sulphur; that which diminishes respirable air. Now inflammable air is that very principle which alone is truly inflammable, as Mr. VOLTA has elegantly shewn. In effect, combustible substances are either animal or vegetable,



as horns, hair, grease, wood, &c.; from all of which Dr. HALES has extracted inflammable air; or charcoal, from which Mr. FONTANA has extracted it, as did Dr. PRIESTLEY from resins, spirit of wine, and æther, in all which it is the only principle that is inflammable, and they are inflammable only in proportion as they yield it; or phosphorus, from whose acid Dr. PRIESTLEY has obtained this air by means of minium, for it was the acid, and not the minium, that contained it, as Dr. PRIESTLEY rightly conjectured, the acid obtained by deliquescence being never thoroughly dephlogisticated until heated and vitrified, as Mr. MARGRAAF has shewn; or they are mineral substances, as sulphur, from which inflammable air has been separated by means of fixed alkalies, and, according to Dr. PRIESTLEY, also by means of marine air, or bitumens or bituminous substances, all of which may be made to yield it; or metallic substances, as zinc and regulus of arsenic, both of which are inflammable; but neither of them is so when deprived of its inflammable air: this is, therefore, the true and only principle of inflammability in any substance. I acknowledge that the inflammable air, proceeding from almost all these substances, is exceeding impure; that it contains from some a mixture of aerial acid or of oil, and from all some part of the substance which yields it or expels it, and hence its smell is different, according to the class of the substances from which it is extracted; but it is equally true, that none of these substances contribute to its inflammability; on the contrary, it is so much the less inflammable (that is, requires so much more air to be mixed with it before it flames) as it contains more of these heterogeneous substances. Hence inflammable air of the morasses is never totally consumed\* ; and, on the contrary,

\* 15 Roz. 146.

inflam-

inflammable air, from metals which is the purest of all, is also the most inflammable.

Secondly, Inflammable air is also the principle which reduces metallic earths to a metallic state, and gives them their metallic splendor. This has been proved analytically and synthetically, and therefore may be said to be as completely demonstrated as any thing in natural philosophy: thus Dr. PRIESTLEY has extracted inflammable air from iron and zinc by heat alone; and the iron, thus stripped of its phlogiston, lost its splendor, and was of a black colour, which is that which iron, slightly dephlogisticated, always assumes, as appears by *marital æthiops*: so also zinc and regulus of arsenic, when once inflamed, lose their metallic appearance: so also a mixture of lead and tin inflames in a moderate heat, and then both are converted into a calx destitute of splendor and malleability. On the other hand, if a current of inflammable air, in the act of combustion, be directed on the calces of iron, lead, or mercury, they are immediately revived and restored to their metallic form, as appears by the experiment of Mr. CHAUSSIER \*. The following experiment is still more conclusive: if a polished plate of iron be put into a saturate and dilute solution of copper in the vitriolic or marine acids (I mention these because they are commonly used for the production of inflammable air, though the result is the same when other acids are used), no effervescence will arise, no inflammable air will be caught; but the iron will be dissolved, and the copper precipitated in its metallic form. Here inflammable air must be produced as usual, for the acid quits the copper and dissolves the iron; but this inflammable air instantly loses its aerial form, and unites to the copper, just as fixed air leaves alkalies to unite to lime

\* 10 Roz. 313.

without.

without any effervescence; and by this same inflammable air is the copper evidently reduced, acquiring splendor, malleability, and every other metallic property. But if the solution of copper be not saturated with copper, a small quantity of inflammable air may be caught, as the excess of acid will disengage more of it from the iron than the calx of copper can take up. Inflammable air is then the principle that metallizes metallic earth; and if metals contain only a specific earth and phlogiston, inflammable air certainly contains nothing else but phlogiston. If iron and the arsenical acid be digested together, no inflammable air is produced; but the arsenical acid is, in great measure, converted into white arsenic, as Mr. BERGMAN has observed, and also Mr. SCHEELÉ\*; what reason can be assigned why inflammable air is not produced by this as well as by all other acids; but that this metallic acid received it, and was by it reduced to a semi-metallic form, as by pure phlogiston? Yet this acid produces inflammable air, from zinc because zinc gives out more phlogiston than the regulus of arsenic can take up; but it attracts and is metallized by a part of it, and it is only the excess that appears in the form of inflammable air, as Mr. SCHEELÉ has remarked. This inflammable air, indeed, is not pure, for it holds some of the regulus in solution; but this portion of regulus does not enter into its composition, and is very evident.

Thirdly, Inflammable air is the substance which, with vitriolic acid, forms sulphur, for it is the very substance which the vitriolic acid separates from metals; and this substance, so separated, when in sufficient quantity, and in proper circumstances, unites to it in such proportion as to form common sulphur. Thus sulphur is formed by distilling concentrated vitriolic acid

\* 2 Nov. Act. Upsal. p. 210. Kon. Vet. Accad. Handlingar, vol. 36. p. 288.

with

with iron or bismuth, or by distilling tartar vitriolate with regulus of antimony. It is this also that diminishes respirable air, as Dr. PRIESTLEY has clearly shewn in the 5th vol. of his Observations, p. 84.; for though in its complete aërial state, after it has absorbed that large quantity of fire requisite to its aërial form, it difficultly and slowly unites to respirable air in the heat of the atmosphere, their points of contact through their difference of density being very small, and there being no substance at hand to receive the large portion of elementary fire they both contain, and of which they must lose a large proportion before they can combine together; yet while inflammable air is (as Dr. PRIESTLEY elegantly expresses it) in its *nascent* state, before it acquires its whole quantity of specific fire, respirable air easily unites to it, and is diminished in proportion to its purity; but if to a mixture of both, igneous particles of sufficient density to be visible be introduced, a degree of heat is excited, which, as it rarifies the dephlogisticated part of respirable air to a greater degree than it can inflammable air\*, brings both into nearer contact, increases their attraction to each other, and both uniting give out their fire, or in other words *inflame*, when in proper proportion to each other; without any decomposition of either, unless the loss of a great part of their specific fire be called a decomposition, which loss is not usually called a decomposition; for water is never said to be decomposed when it becomes ice, nor metals when they become solid on cooling.

In answer to all this it will be said, that inflammable air undoubtedly contains phlogiston, which produces all the before-mentioned effects; but that the phlogiston it contains is united to some other substance, which some will have to be an acid,

some an earth, and others respirable air. To these hypotheses I shall oppose one general observation, which is, that since inflammable air, when pure, that is, when disengaged from all heterogeneous substances which no way contribute to its inflammability, has always the same properties; it must, if it consists of phlogiston combined with any other substance, be always united to the same specific substance; that is, if this be an acid, it must be always the same species of acid, or if an earth, it must be always the same species of earth; for we find, that substances, which are only *generically* the same, always produce, with any other given substance, compounds whose properties are very different from each other. Thus we see that the different species of alkalies, or earths, or metals, produce with one and the same species of acid compounds essentially different. This is a rule which, as far as I know, admits of no exception: and if we apply it to the abovementioned suppositions it will intirely destroy them; for it is impossible to think, that the phlogiston can in every substance, that produces inflammable air, meet either the same acid, or earth, or any respirable air.

But to be more particular, the following reasons demonstrate that an acid of any sort cannot be the basis of inflammable air. 1<sup>st</sup>. Inflammable air has been, by Dr. PRIESTLEY, separated from metals by mere heat. Now metals contain no acid, except perhaps their dephlogisticated calx, which those eminent chemists, BERGMAN and SCHEELE, suspect to be of an acid nature; but these calces cannot enter into the composition of inflammable air, otherwise the inflammable air of each different metal would have different properties, as already shewn: nor indeed are these the acids that have been supposed to enter into the composition of inflammable air; but rather those acids by  
 6 whose

whole means it is extricated. But as this air is extricated from metals, not only by acids, but also by alkalies\*, this supposition must vanish of course.

The same reasons militate with equal strength against the supposition that an earth of any kind enters into the composition of this air; nor is there an instance of any earth rendered permanently fluid by any means, except in sparry air. Besides, if it were a metallic earth, it must necessarily be supposed to be in a metallic state; and how then could it escape the action of all kind of acids? for no acid is capable of decomposing inflammable air. Lastly, respirable air cannot be said to be the basis of inflammable air, unless we suppose that respirable air enters into the composition of metals; for Dr. PRIESTLEY has, by solar heat, extracted inflammable air from them in a vessel full of mercury, into which respirable air had no access, and even *in vacuo*. Besides, respirable air and phlogiston form other compounds very different from inflammable air, *viz.* fixed and phlogisticated airs as will presently be seen.

It may also be fairly urged against all these suppositions, that they are not founded on any direct experiment, nor any known analogy, but merely gratuitous, or at least deduced from experiments inadequate to their support; whereas the opinion that inflammable air is nothing else than phlogiston thrown into a fluid form by elementary fire, is directly founded on that experiment whereby inflammable air is separated from metals by mere solar heat in the most perfect vacuum, just as fixed air united to marble and in a concrete state (in which it is nearly of equal density with gold) is separated from the marble, and thrown into a permanently fluid form by heat alone.

\* Mem. Par. 1776, p. 687.

In favour of the existence of an acid in inflammable air, it has been said, that if this air be passed through water tinged blue by litmus, it reddens instantly. I have seen this frequently happen when inflammable air has been extracted from iron by spirit of vitriol; but if this air be washed, by passing it through lime-water, and then passed through, or agitated in, an infusion of litmus, it will not discolour it in the least: this I have seen done by Mr. FONTANA in June 1779. It has also been said, that inflammable air and alkaline air, mixed together, form a cloud; but this has been fully disproved by Dr. PRIESTLEY, in the fourth volume of his *Observations*.

That an earth of any kind is essentially requisite to the constitution of inflammable air, seems to me utterly improbable; nor do I know of any experiment from whence it can be inferred. That metallic substances may be held in solution by inflammable air is certain; but it is equally so, that they no way contribute to its inflammability\*, and are quite distinct from it.

But the opinion, that inflammable air consists of respirable air super-saturated with phlogiston, is grounded on very specious arguments drawn from experiments to be found in various parts of Dr. PRIESTLEY's works, which deserve so much the more attention as the facts mentioned by that excellent philosopher are not to be questioned. I shall endeavour to state them with accuracy; but shall at the same time accompany them with such remarks as seem to me to invalidate the conclusion that has been drawn from them.

In the first volume of Dr. PRIESTLEY's *Observations* it appears, that a quantity of strong inflammable air, having been agitated in a glass jar immersed in a trough of water,

\* 2 PRIESTLEY, 268.

the surface of which was exposed to the common atmosphere, after the operation had continued ten minutes near one-fourth of the quantity had disappeared; the remainder became fit for respiration, and yet was weakly inflammable. By further agitation it was diminished half, and then admitted a candle to burn in it though feebly; but, on continuing the agitation a little longer, it came to extinguish a candle. Upon this I shall remark, first, that it clearly follows, from this experiment, that if the external respirable air had no access to the inside of the jar, half nearly of the inflammable air was converted into, or consisted of, respirable air, since such quantity of air was found in it after the operation. Now it is absolutely impossible that either could happen; for inflammable air could not be converted into half nor even one-third or one-fourth of its volume of respirable air, as even one-fourth of respirable air contains more matter than four times its bulk of inflammable air; it is then evident, that the external air must have had access to it. Secondly, I agitated about half a pint of inflammable air, obtained from iron and previously passed through lime-water and kept over mercury, in about twelve times its bulk of water, out of which its air had been boiled in a glass bottle closed with a glass stopper. The agitation continued at several times at least two hours. A large quantity of the air was indeed absorbed, as appeared by opening the bottle in water; but the remainder appeared, by the nitrous test, as noxious, and was also found to be as inflammable as at first. Even Dr. PRIESTLEY attests, that inflammable air, which had been united to water for one month, was afterwards as inflammable as ever. 3 PR. 267.

The true explanation of the first experiment appears, therefore, to be the following: First, Water easily imbibes inflammable air, but does not combine with it; for after it has imbibed



bided one-fourteenth of it, its taste is no way altered, as Dr. PRIESTLEY has observed. 1 PR. 196. Water also easily imbibes common air: when, therefore, inflammable air is agitated in water having a communication with the atmosphere, the inflammable air must necessarily be diminished by reason of its absorption, and the part so absorbed immediately escapes out of the water into the atmosphere, as is evident by the smell which is perceived when the quantity of inflammable air is considerable. This escape gives room for the further absorption of the inflammable air which then escapes in the same manner. In the mean time the common air under the jar rises into it, as appears by the direct experiments both of Dr. PRIESTLEY \* and Mr. FONTANA; and hence the air in the jar must appear by the nitrous slightly phlogisticated and respirable; but a further agitation will decompose the common air, as we shall soon see, and then a candle will be extinguished. The same process takes place when inflammable air stands long in water whose surface is exposed to the atmosphere.

Another experiment of the same tendency, but seemingly more decisive, is to be found in the 4th vol. of Dr. PRIESTLEY's Observations, p. 368. There it is related, that a portion of inflammable air, inclosed in a glass tube, hermetically sealed and heated until the glass was softened, stained the glass black, and the tube being opened, the air was found reduced to one-third of its bulk; and this residuum was found to be mere *phlogisticated air*, neither precipitating lime-water, nor being affected by nitrous air, or in the least inflammable. Yet decisive as this experiment appears, a little consideration will shew the absolute impossibility that inflammable air should consist of one-third phlogisticated air and two-thirds phlogiston;

\* 1 PR. 96. 159. 3 PR. 156. Phil. Trans. 1779, p. 443.

for,

air, in the first place, one Cubic inch of phlogisticated air weighs 0,377 of a grain: now let us suppose, that to this phlogisticated air is added two-thirds of its bulk of phlogiston; and to make the supposition still stronger, let us also suppose, that phlogiston has no weight; then, by the supposition, this compound of phlogisticated air and phlogiston will constitute inflammable air, and amount to a bulk of three Cubic inches, and these three Cubic inches will weigh no more than 0,377 of a grain; but if three Cubic inches of inflammable air weigh 0,377 of a grain, one Cubic inch should weigh 0,125 of a grain, which cannot be; for then inflammable air would be little more than one-third lighter than common air, contrary to all the experiments that have been hitherto made, and particularly those of Mr. CAVENDISH, FONTANA, and Dr. PRIESTLEY himself, which shew it to be about eleven times lighter than common air. Secondly, It is said, that the matter which stained the glass black was the true phlogistic part of inflammable air, and was afterwards separated by means of minium. This then contained no phlogisticated air; but is it not certain, that if there had been enough of it, the minium would have been reduced and converted into lead? And might not inflammable air be again separated from that lead, though no phlogisticated or common air were at hand to supply its other supposed constituent part? Thirdly, In one of Dr. PRIESTLEY's experiments the inflammable air, contained in the glass tube which was most heated, was reduced to so small a bubble that no experiment could be made on it: therefore, in this, at least, the quantity of phlogisticated air did not amount to one-third, but was quite inconsiderable; the remainder then being taken up by the calx of lead in the glass, was pure mere phlogiston, so that this experiment is a strong proof of my opinion. Fourthly,

Fourthly, If phlogiston could be decomposed by heat, and then leave a residuum of phlogisticated air, amounting to one-third of its bulk, the diminution arising from its inflammation with common or dephlogisticated air could never be so great as it is found to be by repeated experiments; for when inflammable and common air are fired in the proportion of eleven of the latter to four of the former, a bulk equal to the whole of the inflammable air, and to one-fifth of the common air, disappears, according to Mr. VOLTA \*, and the diminution is about two-fifths of the whole, or more exactly out of fifteen measures, only 8,8 remain; but if the inflammable air were decomposed, and one-third of it, being phlogisticated air, should remain, then not quite one-fifth of the whole would vanish, and the residuum should be 10,54 measures. This evidently proves, that pure inflammable air is never decomposed (unless the loss of its fire be called a decomposition); but in the act of inflammation is totally transferred upon the pure part of respirable air to which it unites. Fifthly, To obtain still a clearer insight into this matter, I intreated Mr. CAVALLO, who is very expert in the management of the blow-pipe, as well as in pneumatic experiments, to repeat this experiment in my laboratory. We accordingly filled a tube 10,5 inches long, and one-fourth of an inch in diameter, with inflammable air from iron received over mercury, and having made the tube red-hot throughout and black, and softened it so far as to endanger the escape of the air, we opened it on mercury. The air was diminished only one-tenth, and inflamed with an explosion as loud as an equal quantity of the same inflammable air that had not been heated.

\* Roz. April 1779, p. 295.

The only question that remains then is, whence the phlogisticated air proceeded which Dr. PRIESTLEY mentions to have found? The circumstance of his experiment would furnish a plausible answer; but the doctor has lately informed me, that he believes the air was really inflammable, but being a very small quantity escaped before the flame could be applied.

It seems, therefore, sufficiently proved, that inflammable air purified from the acids or other substances that expel it from its basis, and also from all particles of the body to which it was originally united, such as inflammable air from metals received on mercury, and well washed in lime-water, is one and the same substance with phlogiston, differing only in quantity of fire, inflammable air containing nearly the same quantity of this element as the same bulk of atmospheric air, as Dr. CRAWFORD has found by some late experiments, an account of which will soon be laid before the public. This does not contradict that most important discovery of this ingenious philosopher, that fire and phlogiston repel each other: the meaning of this being only, that the addition of phlogiston to any substance, as to respirable air, dephlogisticated acids, metallic calces, expels part of the fire already contained in such substance; and, on the contrary, by the removal of phlogiston from any substance, the quantity of fire absorbed by such substance is increased.

It may appear extraordinary, supposing inflammable air and phlogiston to be the same substance, that inflammable air should mix so easily with water, whereas phlogiston constantly repels and is repelled by it; but this intirely depends on the state of this same substance, which, when fixed and concrete, is called *phlogiston*, and, when rarified and aëriform, *inflammable air*. In this latter state it mixes with water in proportion to its rare-

faction, as it even does in the less dense forms of its concrete state: thus æther is totally absorbed by ten times its weight of water. The animal oil of Dippel mixes intirely with water; so does pure Petrol, and essential oils frequently distilled, and the spiritus rector of plants.

Much more remains to be said of the different states of phlogiston from its most rarefied known state, viz. that of inflammable air, to its most condensed state, that in which it is combined with metallic earths, &c. I have already distinguished eight intermediate states each differing from the other by the portion of elementary fire they contain, this quantity being, as far I can judge directly, as the rarefaction of the phlogiston; but these researches are foreign to my present subject. I shall only remark, that phlogiston, in a state perhaps 100 times rarer than inflammable air, and consequently containing much more fire, may possibly constitute the electric fluid.

P. S. Since I wrote the above, I have been honoured with a letter from Dr. PRIESTLEY, in which he informs me, that he has reduced the cakes of iron, copper, lead, and tin, merely by melting them in inflammable air by means of a burning glass. A certain quantity of inflammable air was absorbed by each during their reduction; but the unabSORBED part was equally inflammable, so that there was no decomposition; but the remainder was of the same nature as the part absorbed. He also, by the same means, converted nitrous vapour into nitrous air, and the phosphoric acid into phosphorus. And since the communication of the last mentioned experiments, which seem to him also a direct proof of the identity of inflammable air and phlogiston, he has been so obliging as to inform me, that he has revived the cakes of metals in *alkaline air* as well as

in inflammable air, and also formed a phosphorus; and that he has little doubt but that he shall be able to produce any thing else in which phlogiston is supposed to be concerned. This, he says, agrees with several of his former experiments, especially that in which he produces inflammable air from alkaline air, by means of the electric spark and volatile alkali from iron, supersaturated with phlogiston by means of nitrous air, which he has repeatedly done since the publication of his last volume. This observation, he adds, may help to explain some things in the theory of chemistry, especially the affinity which all acids have both with phlogiston and with alkalies; but, he says, that alkaline air contains something else besides phlogiston; because when this air is used, there is always a residuum of something that is neither alkaline nor inflammable air; but he wants more sunshine to complete and extend his experiments on this subject\*.

## OF THE QUANTITY OF PHLOGISTON IN NITROUS AIR.

100 gr. of filings of iron being dissolved in a sufficient quantity of very dilute vitriolic acid produced, with the assistance of heat gradually applied, 155 cubic inches of inflammable air, the barometer at 29.5, and the thermometer between 50 and 60°. Now inflammable air and phlogiston being the same thing, this quantity of inflammable air amounts to 5.42 gr. of phlogiston.

Again, 100 gr. of iron, dissolved in dephlogisticated nitrous acid, in a heat gradually applied and raised to the utmost, afford 83.87 cubic inches of nitrous air. And as this nitrous air con-

\* Since this paper was committed to the press, I find that Mr. PELLETIER has reduced the arsenical acid to a regular, by merely passing inflammable air through the solution of that acid in twice its weight of water. *Mem. Journ.* February 1782.

tains nearly the whole quantity of phlogiston which iron will part with (it being more completely dephlogisticated by this acid than by any other means) it follows, that 83,87 cubic inches of nitrous air contain at least 5,42 gr. of phlogiston; but it may reasonably be thought that the whole quantity of phlogiston which iron will part with is not expelled by the vitriolic acid, and that nitrous acid may expel and take up more of it. To try whether this was really so, I calcined a certain quantity of green vitriol, until its ferruginous basis was quite insipid; I then extracted from 64 gr. of this ochre two cubic inches of nitrous air, consequently 100 gr. of this ochre would give 3,12 cubic inches of nitrous air; and if 83,87 cubic inches of nitrous air contain 5,42 of phlogiston, then 3,12 cubic inches of this air contain 0,2 of a grain of phlogiston; consequently, nitrous acid extracts from 100 gr. of iron two-tenths of a grain more phlogiston than the vitriolic acid does; therefore 83,87 cubic inches of nitrous air, containing nearly all the phlogiston which iron gives out, contain 5,62 gr. of phlogiston.

Then 100 cubic inches of nitrous air contain 6,7 gr. of phlogiston, and since 100 cubic inches of nitrous air weigh 39,9 gr. they must also contain 33,2 gr. of nitrous acid.

Also, 100 gr. of nitrous air contain 16,792 of phlogiston, and 83,208 of acid.

When first I made these experiments I imagined, that the nitrous air thus expelled contained all the phlogiston of the metals dissolved in the nitrous acid, as this acid is well known to dephlogistate metals as perfectly as possible; but I soon observed, as did Dr. PRIESTLEY and Mr. FONTANA, that the greater part of this is air resorbed and detained in the solution, the acid and calx having, according to the beautiful remark of Mr. SCHEELE, a greater attraction to phlogiston than either separately; yet that the calculation

calculation is nearly just, will appear clearly in my next paper, by its coincidence with the quantity of phlogiston discovered in lead by Dr. PRIESTLEY and that which is contained very evidently in regulus of arsenic, silver, and quicksilver.

## OF THE QUANTITY OF PHLOGISTON IN FIXED AIR.

Before I attempt to determine this quantity, it will be necessary to prove that it contains any; and for this purpose minutely to examine its nature and origin.

Dr. PRIESTLEY first discovered that in all processes, wherein phlogiston is disengaged from any substance, as in *combustion, respiration, calcination of metals, putrefaction, decomposition of nitrous air by respirable air, &c.* fixed air is precipitated from the common or dephlogisticated air in which these processes are performed, and that these last airs are diminished both in weight and bulk, and are afterwards less fit, or absolutely unfit, for these processes, according to the quantity of phlogiston that was set loose. These facts are admitted by all, let their systems be what they may. However, Dr. PRIESTLEY thinks he has seen one exception to this general rule; for, he says, that in the combustion of inflammable and common air no fixed air is precipitated, 5 PR. 124. He also seems inclined to admit another exception in the case of the combustion of sulphur.

The questions that here arise are, first, whether the fixed air that appears in these circumstances proceeded from the respirable air or not? Secondly, If it proceeded from the respirable air, whether it pre-existed in that air; or whether it was generated:



#### § 14. *Continuation of the Experiments and Observations.*

generated during the process that exhibits it? and if so, what are its constituent parts?

The first question is easily answered; for in such phlogistic processes as are attended with the destruction of the substances that are known to contain fixed air, as those of the animal and vegetable kingdom, the fixed air may be supposed to proceed in many cases, both from the decomposed substance and from the respirable air; and of this sort are the processes of combustion of most animal and vegetable substances, and fermentation; but the fixed air, that appears in such phlogistic processes as are performed on substances that contain no fixed air, must be deemed to proceed from the respirable air singly. And of this case we have four clear instances; the calcination of metals; the decomposition of nitrous air by respirable air; the diminution of common air by the electric spark; and, lastly, its diminution by amalgamation.

And first as to the calcination of metals, Dr. PRIESTLEY has observed, that by this operation respirable air (and only respirable air) is diminished between one-fourth and one-fifth, both in its weight and bulk; but Mr. LAVOISIER has demonstrated, that nothing is lost or escapes through the vessels (as Mr. SCHÉELE would have it); for the weight and materials continue undiminished when the operation is performed in close vessels\*. That part, therefore, which the air loses is taken up by the metallic calx, which accordingly is found to gain the very weight which the air loses. Now the air contained in the calx is fixed air; for Mr. LAVOISIER also observed, that by the calcination of lead, by solar heat, over lime-water, the water was rendered slightly turbid†. It is true,

\* Mem. Par. 1774.

† I LAVOIS. 291.

that

that Dr. PRIESTLEY, in a similar experiment, did not observe this turbidity; but he accounts for this circumstance very justly, by supposing, that the calx of lead absorbed the fixed air preferably to the lime. And this supposition is not gratuitous; for metallic calces, and particularly those of lead, are known to attract fixed air as strongly as quick lime, or rather more strongly\*: and what sets this matter beyond all doubt, the calces of lead all yield fixed air by heat, and the grey calx of lead, in particular, which was that produced by Dr. PRIESTLEY, in the experiment to which I allude, affords by heat fixed air only. Other calces of lead after fixed air afford also dephlogisticated air; but this I shall shew also to have been originally fixed air. If filings of iron be mixed with water in close vessels, they will be converted into rust, and the incumbent air diminished one-fourth, as Mr. LAVOISIER attests†; but Dr. PRIESTLEY has shewn, that rust of iron yields scarcely any other than fixed air, which may be expelled out of it by mere heat‡. Nay, iron alone, exposed to common air over a vessel of water for three months, reduced this air one-fifth; and being exposed to dephlogisticated air, over a vessel of mercury, it reduced it one-tenth in nine months§. In all these cases the fixed air could surely come from nothing else but the incumbent respirable air and the phlogiston of the metal.

Secondly, It is well known, that if nitrous air be decomposed by respirable air over lime-water, the lime will be precipitated||. In this case also, the fixed air must proceed from the

\* VOGEL, § 599. 2 N. Act. Upsal. 240. IX Mem. Scav. Etrang. 544.

† LAVOIS. 192.

‡ 2 PR. 112.

§ 2 PR. 182. 4 PR. 253.

|| 1 PR. 114. 3 PR. 30. 1 PR. 138.

respirable

respirable air and the phlogiston of the nitrous air; for it cannot proceed from the nitrous acid, as this acid is not decomposed, but is taken up by the water over which the mixture of both airs is made, as Mr. BEWLY has undeniably proved: and hence it is, that unless a large quantity of lime-water be used so as to contain enough for both the nitrous and aërial acids to act on, there will be no precipitation of lime, as Mr. FONTANA has observed; for the nitrous acid will seize on the lime preferably to the aërial. Dr. PRIESTLEY indeed observed, that if a bladder, filled with nitrous air, be dipped in lime-water, it occasions a precipitation of lime on the surface of the water. 1 PR. 213. But he elsewhere acknowledges, that this proceeds from the inability of the bladder to confine nitrous air. 1 PR. 76. and 128, which Mr. BAUME also long ago observed, without knowing any thing more of this air. BAUME *sur l'Æther*, 285. The phlogiston passes through the bladder, and unites to the common air contiguous to it\*. Besides, nitrous air acts on the bladder itself, and extracts fixed air from it. 1 PR. 214. Hence also, if rain-water carefully boiled, and freed from its own air, be made to absorb a quantity of nitrous air, it will again, on boiling, yield it back as pure as at first; but if common water be made to imbibe nitrous air in the same manner, it will, on boiling, yield also a portion of fixed air. 3 PR. 109. Does not this happen clearly because common water contains atmospheric air, or air somewhat purer, which is converted into fixed air by mixture with the nitrous air? This experiment also shews, that water itself never unites to phlogiston, since it does not take any from nitrous air, where the union of phlogiston to the acid is of the laxest kind.

\* 3 PR. 156.

Thirdly, If the electric spark be taken through common air, this air will be diminished one-fourth, and a solution of lime, if contiguous, will be precipitated, and a solution of turnsole tinged red. 1 PR. 184. 186. Whence could the fixed air here produced proceed, but from the common air, and the phlogiston of the metallic conductors? This excellent philosopher has even shewn it could proceed from nothing else; for after that air had contributed all it could to that production, that is, was diminished to the utmost, he changed the liquors, but could produce no change in their colour, nor the least sign of fixed air. This experiment has also been repeated in France, and the inside of the glass tube, in which the common air was contained, was moistened with a solution of caustic fixed alkali, and the alkali, after the operation, was found crystallized; but when the tube was exhausted of air, and the experiment repeated, no change whatsoever was found in the alkali. *Essai sur l'Electricité, par Mr. Le Comte DE LA CÉPEDE*, vol. I. P. 155.

Fourthly, If lead and mercury be agitated in a phial, partly filled with common air, this air will be diminished one-fourth, and the residuum will be found completely phlogisticated. The diminution will be still greater if the phial contain dephlogisticated air. 1 PR. 149. The lead is converted into a calx, calcination being the known effect of the amalgamation of the base metals; and this calx absorbed the fixed air produced, for Dr. PRIESTLEY expelled this air from it. 1 PR. 144.; and hence an amalgama of lead and mercury decrepitates when heated\*. Whence could this fixed air proceed, but from the respirable air? For surely neither lead nor mercury contain any.

\* 1 MALOUIN. 105.

If the above experiments be attended to, the answer to the second question will be equally obvious. It is certain, that common air does not consist of one-fourth of its bulk of fixed air; for if it did, the remaining three-fourths must be dephlogisticated air: and if so, then the absolute weight of a mixture of three-fourths dephlogisticated air and one-fourth fixed air should coincide at least nearly with the absolute weight of an equal bulk of common air; but in fact it is very far from it: for four cubic inches of common air weighed 1,54 gr.; but a mixture of three cubic inches of dephlogisticated air and one of fixed air weighs 1,83 gr.; neither indeed has so large a portion of fixed air been ever supposed to exist in common air. Besides, if fixed air pre-existed in common air, it might be separated from it by lime-water, at least in some degree. I have mixed one part of fixed air with twenty of dephlogisticated air, and also with twenty of phlogisticated air in close vessels, and these mixtures did not fail to render lime-water turbid. But let common air be agitated in lime-water ever so long in close vessels, not the least cloudiness will appear; nor does quick-lime, in these circumstances, in the least affect common air, as Dr. PRIESTLEY has observed. 2 PR. 184. The spontaneous precipitation of lime-water arises therefore from an accidental diffusion of fixed air through common air, and the slowness of this precipitation shews its quantity to be very small. The inference from the above experiments will be much stronger against the pre-existence of fixed air in respirable air, if, instead of common air, dephlogisticated air be used; for there the diminution is so great, and the quantity of fixed air produced so considerable, that it can by no means be supposed to have pre-existed, its properties being so very opposite to those of dephlogisticated air.

To

To this it has been answered, first, that fixed air in common air is united to some unknown basis, which attracts it more strongly than quick-lime does; but that it is precipitated from that basis by the phlogiston set loose in phlogistic processes, which is still more strongly attracted by that basis; and, secondly, that the diminution both of the weight and bulk of respirable air in phlogistic processes does not arise intirely from the separation of fixed air, but from some other cause.

But neither of these answers is satisfactory: for the supposition of such a basis is evidently gratuitous, being supported by not one experiment. It is also contrary to analogy, there being no instance of the separation of fixed air, nor of any other acid, from any substance merely by the greater affinity of phlogiston to such substance. It is also insufficient for the purpose for which it was framed; for of dephlogisticated air 97 parts in 100 are reducible to fixed air by phlogistic processes; and can it be imagined, that 97 parts in 100 of it were mere fixed air united to less than three parts of an unknown basis? I say, less than three parts; for, according to the present supposition, this unknown basis took up the phlogiston of the substance that separated the fixed air from it, and yet it, and the whole quantity of phlogiston it took up, amounted but to three parts of an hundred; can it be supposed, that this vast proportion of fixed air would not in the least affect lime-water, as pure dephlogisticated air is known not to do? Can it be supposed, that such an immense quantity of fixed air, combined with any basis, would be so superlatively fitted for all phlogistic processes, while fixed air, in its disengaged state, is totally unfit for them? Besides, this unknown basis, after all, is nothing but phlogisticated air, with which fixed air is incapable of contracting any union; and if its phlogiston be washed away, it is not

found different from common air slightly injured. Accordingly, we find that this conjecture, first advanced by Dr. PRIESTLEY in the infancy of his researches, is now abandoned by him. Vol. V. p. 31. And he now justly thinks, that common air does not contain above  $\frac{1}{12}$  of its bulk of fixed air.

As to the diminution of bulk, it is certain, that the whole of it does not proceed from the separation of fixed air; for though no part of the fixed air should be absorbed, yet since part of the common air is converted into fixed air, there must be a diminution of bulk, since fixed air is specifically heavier than common air, and the bulks are inversely as the specific gravities; but the diminution of *mass* must wholly, and that of bulk must also for the greater part arise from the absorption of fixed air by water, or the substance from which the phlogiston proceeds. I have successively added six measures of nitrous air to two of dephlogisticated air from precipitate *per se*, and after each addition transferred the mixture into fresh lime-water, and after each I found the lime precipitated until the whole was reduced to one-tenth nearly, so that nine-tenths of this dephlogisticated air was evidently converted into fixed air; and since fixed air did not pre-exist in the dephlogisticated air, it was evidently produced by the union of the phlogiston of the nitrous air with the truly dephlogisticated part of the dephlogisticated air.

Here we see how fixed air is generated in most other phlogistic processes, performed in common air. The phlogiston is attracted by the dephlogisticated part of common air, unites to it, expels part of its fire, and so forms fixed air; yet a part of this pure air generally escapes the action of phlogiston, being protected from it by the quantity of phlogisticated air which is always found in common air, and which forms about two-thirds of it, in the same manner as gold is protected by silver,

silver, and silver by gold, from the action of their respective menstruums; and this is the reason why, in some phlogistic processes, the diminution is greater than in others; and why the diminution continues to increase slowly for a long time.

Nor is the supposition, that common air consists of two fluids, one phlogisticated and the other dephlogisticated, gratuitous; it is pointed out by several experiments. If a mixture be made of three parts phlogisticated air and one of dephlogisticated air, it will exactly perform the functions of common air; a candle will burn in it, an animal will live in it, just as in common air\*. Besides, common air may in some measure be separated into these constituent parts by lying over pure water; for dephlogisticated air is much more miscible with water than common air, as Mr. FONTANA remarked, *Phil Trans.* 1779, p. 443. and 444†, and SCHEELE on Fire, § 94. Hence, if common air be suffered to stand some time over pure water, it will be diminished, the purer part being in great measure absorbed by the water, and the remainder will be found to consist of so large a proportion of phlogisticated air that a candle will not burn in it. 1 PR. 158. 4 PR. 353. Mr. SCHEELE again expelled that part which the water had absorbed, and found it dephlogisticated. He also found, that phlogisticated air is not at all absorbed by water. *ibid.*

Hence we see, why the whole of any quantity of common air can never be converted into fixed air; for no part of it will unite with phlogiston, but the dephlogisticated part (which never exceeds one-third of the whole). This Mr. SCHEELE has decisively proved by exposing liver of sulphur to a mixture

\* *Mem. Par.* 1777, p. 191.

† He informed me, that water takes up one-fourteenth of its bulk of dephlogisticated air, and only one-twenty-eighth of common air.



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of phlogificated and dephlogificated air; the mixture was diminished in the same proportion as it contained dephlogificated air, and no more. SCHEELE, § 43.

Phlogificated air, therefore, is not the usual product of common phlogistic processes; but the phlogificated residuum that is found after such processes must have pre-existed, as that evidently does which is found after the mixture of nitrous and very pure dephlogificated air, for almost the whole of this last is turned into air which is absorbed by water, and precipitates lime, as we have already seen, so that no part of it is converted into phlogificated air, this being immiscible with water. Now common air is affected by nitrous air just in the same manner, and differs only in degree; therefore the phlogificated air, which is found after its phlogification in the usual processes, was not produced by those operations, but pre-existed.

Phlogificated air consists of fixed air super-saturated with phlogiston, as sulphur does of volatile vitriolic acid super-saturated with phlogiston; and as sulphur is not generally formed when the vitriolic acid unites to phlogiston, but only volatile vitriolic acid, so neither is phlogificated air each time that pure air unites to phlogiston, but rather fixed air, I say *super-saturated*, because it contains such a quantity of phlogiston as to be insoluble in water. Many experiments of Dr. PRIESTLEY clearly point out this composition. Thus that celebrated philosopher has found, that if phlogificated air be agitated in water, out of which its air had been boiled, and whose surface is exposed to the atmosphere, it will be in great measure purified (just as sulphur is decomposed by trituration in water), and if then it be passed through lime-water two or three times, it renders it turbid. 2 PR. 218. Here then the excess of phlogiston, by reason of its repulsion from water, is easily

easily attracted by the dephlogisticated part of the common atmosphere, which is immediately imbibed by the water out of which its air had been boiled; the phlogisticated air is thereby decomposed and partly turned into fixed air, which makes the lime-water turbid. Part also of the fixed air is decomposed as will presently be seen, and hence the degree of purity which it acquires. Further, if the electric spark be taken in fixed air, three-fourths of it will be rendered insoluble in water, and the whole will probably become so if the operation be long enough continued. 1 PR. 248. This insoluble residuum Mr. FONTANA found to be phlogisticated air; and that, if this phlogisticated air be agitated in water (whose surface is exposed to the atmosphere) it will become again common air. *Recherches Physiques*, 77. That is, it will acquire a degree of purity nearly the same as that of common air. This fully confirms all that has been hitherto said concerning these airs; so also, if a mixture of filings of iron and sulphur, made into a paste, be exposed to fixed air, and made to ferment, part of the fixed air will be turned into phlogisticated air, 3 PR. 257. Just as in another equally curious experiment he found, that vitriolic air was converted into sulphur by the gradual exhalation of phlogiston from a solution of that air in water, and as it daily happens in the hot baths at Aix la Chapelle. And hence we see, that fixed air, even in its elastic state, is capable of taking an excess of phlogiston, when this last is insensibly separated from any substance, and then becomes phlogisticated air. Phlogisticated air may also be formed by a rapid and copious affluence of phlogiston, in certain circumstances, as we shall soon see. I should not omit, that phlogisticated air, after it has been purified from phlogiston by agitation in water, is again diminishable by phlogistic processes, and that fixed air is precipitated

pitated from it as usual. 2 PR. 219. A circumstance which at that time was thought inexplicable, and which indeed is so, on any other principles but those here laid down, of which it is an immediate consequence.

Having thus far synthetically proved the constituent parts of fixed air to be pure elementary air and phlogiston, I shall now endeavour to do the same by its analysis: and, in the first place, that it contains phlogiston, and even in such quantity as to deserve to be classed among the phlogisticated acids, appears by its action on *black manganese*. This semi-metallic calx, as has been proved by that admirable chemist Mr. SCHEELÉ, is completely soluble only in phlogisticated acids, and is precipitable from them by fixed alkalies in the form of a white calx. He also found, that this manganese is also soluble in water strongly impregnated with fixed air, and is also precipitable from it in the form of a *white calx*. 35 Mem. Stock. p. 96.

If fixed air be repeatedly dissolved in, and expelled from water, it leaves each time a residuum which is insoluble in water, diminishable by nitrous air, and capable of supporting animal life. Hence it is evidently decomposed, the phlogiston separating from it, and gradually uniting to the common atmosphere by reason of the repulsive power betwixt it and water. Dr. PRIESTLEY indeed found, that a candle would not burn in it; but this arises only from a mixture of a small quantity of fixed air not yet decomposed, of which, according to the experiments of Mr. CAVENDISH, one-ninth is sufficient to extinguish a candle\*.

Again, Mr. ACHARD has converted fixed air into air of nearly the same purity as common air by passing it five or six times through melted nitre. Mem. Berlin. 1778. Mr. CAVALLO

\* 1 PR. 34. 40. 2 PR. 219, 220.

passed it but once through melted nitre, and yet found it considerably meliorated, for it was diminished by nitrous air. In this case the nitrous acid attracted the phlogiston; for it is known to become phlogisticated by the fusion of nitre, so as to be expellable even by the vegetable acids. 2 N. Act. Upf. 171. And aqua regia may be made by mixing nitre with marine acid.

I shall now briefly consider what may be said in opposition to this doctrine.

In the first place it may be difficult to conceive, that the addition of phlogiston should render any substance more soluble in water, as it is known to render most acids less soluble in that liquid; but a little attention will shew, that phlogiston does not always render substances less soluble in water than they were before; for the acid of sugar is less soluble in water than sugar itself, though sugar consists of that acid united to phlogiston. The dephlogisticated marine acid unites more difficultly with water than the same acid does when phlogisticated, as the illustrious BERGMAN has observed\*. Caustic volatile alkali has been decomposed by Mr. SCHEELÉ, and found to consist of an air insoluble in water, and phlogiston; so that it is rendered soluble in water only by union with phlogiston. It would be foreign to the subject to enter into the reason of these exceptions, but the facts are certain.

Another objection may be drawn from a remarkable experiment to be found in the fifth volume of Dr. PRIESTLEY's observations, where it is said, that inflammable air and common air being fired by an electric spark over lime-water, the diminution took place all once, and the lime was not precipitated; but as it is equally true, that fixed air is precipitated by other phlogistic processes, this experiment proves only, that in these

\* Anleitung, § 333.

particular circumstances, where a large quantity of phlogiston is suddenly heated and transferred all at once upon the dephlogisticated part of common air, phlogisticated air may be formed as sulphur and volatile acid is formed, when a large quantity of hot phlogiston is united all at once to the vitriolic acid.

Analogous to this is another experiment of Mr. CAVALLO's, where he found, that, by the explosion of gunpowder, a large quantity of phlogisticated air is produced \*; and also another of Dr. PRIESTLEY's, wherein he found, that by firing a mixture of equal parts, sulphur and nitre, only one-twelfth of the air produced was fixed air, the remainder being phlogisticated air. But I own the circumstances of the former experiment are not as yet well known to me, not having been able to repeat it in such a manner as to remove all doubt either of the escape of the air through the cement which fixes the wire that conducts the electric fire, during combustion; or that the small quantity of inflammable air used prevents the fixed from being sensible. It may also happen, that to the production of fixed air it is necessary that the phlogiston be condensed to a certain degree, as it is in common cases; and perhaps, when exceedingly rarified, as it is in inflammable air from metals, it forms some other, as yet unknown, compound. Thus much is certain, that all other inflammable air, fired with the electric spark, produces fixed air; and all other inflammable air is specifically heavier than the metallic, and before inflammation evidently contains no fixed air. Mr. WARLTIRE, after burning metallic inflammable air, found a *white powdery substance* (probably a calx) which may have absorbed the fixed air.

However, in the common process of combustion of animal and vegetable substances it is certain, that fixed air is separated

\* CAVALLO on Air, p. 812.

from

from common air, and that the whole diminution is owing to its production and absorption. Mr. LAVOISIER has set these points in the clearest light. He introduced a lighted candle into a receiver standing on mercury: the air at first expanded by reason of the heat, and the candle was shortly after extinguished; but when all was cold, there was scarcely any diminution. He then introduced, under the receiver, a caustic fixed alkaline liquor: the air was immediately diminished, and the diminution reached nearly one-ninth of the whole. He then introduced a small quantity of vitriolic acid; the alkali immediately effervesced, gave out its fixed air, the mercury re-descended, and the air in the receiver occupied the same space as at first; so that this experiment is perfectly conclusive. He also lighted a candle in dephlogisticated air, and when it was extinguished, introduced a caustic fixed alkaline liquor, and then, and only then, this air was diminished two-thirds, by which it is evident, that two-thirds of it were converted into fixed air; but the remaining third was so far from being phlogisticated air, that a candle burned in it as well as ever, and after it went out half of this air was absorbed by a caustic fixed alkali, and the remainder was still little worse than common air. *Mem. Par. 1777, p. 195, &c.*

Yet Mr. LAVOISIER thinks, that by the calcination of metals fixed air is not produced; but that the metals absorb the dephlogisticated part of common air, and are thereby converted into a calx. And on this is founded his extraordinary opinion of the non-existence of phlogiston; whereas it is evident, that even mercury affords inflammable air, and consequently contains phlogiston, and that it loses part of this during calcination, and consequently fixed air must be produced, as he himself acknowledges it to be during combustion, by the union of inflam-

mable air and the dephlogistified part of common air, which after this union is absorbed by the calx. It is true, that the mercurial calx, and also the calces of lead, and many others, yield dephlogistified air; but then the mercury is always revived, so that it is evident, it retakes the phlogiston from the fixed air, of which nothing then remains but the dephlogistified part, which accordingly appears in the form of dephlogistified air. Dr. PRIESTLEY never found the whole of the mercury revived, and accordingly he recovers a little fixed air from the mercurial calx. 2 PR. 217. But Mr. LAVOISIER finds the whole of the mercury revived, and for that reason finds no fixed but all dephlogistified air; thus their different results are clearly explained, and probably proceed from the different degrees of heat they employed, and the different phlogistification of their acids. The dephlogistified air that is extracted from *minium* proceeds also from a partial revivification of the lead, which always takes place\*: nor is it wonderful, that this calx should dephlogistify fixed air, since it dephlogistifies the marine acid also, as Mr. SCHEELÉ has observed †.

To this it will probably be objected, that dephlogistified air must pre-exist in the *minium*, since it is expelled by the marine acid; but this does not follow; for if manganese be dissolved in the common marine acid which is phlogistified, and afterwards expelled from it by the vitriolic, it will also be found dephlogistified.

I shall now proceed to investigate the proportion of phlogiston and elementary or respirable air in fixed air.

Dr. PRIESTLEY, in the fourth volume of his *Observations*, p. 380. has satisfactorily proved, that nitrous air *parts* with as

\* BEAUME, 7. 1 Pott. Lithog. 29. 3 Dict. Chy. 205.

† Kœn. Vet. Acad. Handling. vol. XXXV. p. 293.

much

much phlogiston to common air as an equal bulk of inflammable air does when fired in the same proportion of common air. Now, when inflammable air unites with common air, its whole weight unites to it, as it contains nothing else but pure phlogiston; since, therefore, nitrous air phlogisticates common air to the same degree that inflammable air does, it parts with a quantity of phlogiston equal to the weight of a volume of inflammable air similar to that of nitrous air. Now 100 cubic inches of inflammable air weigh 3,5 gr.; therefore, 100 cubic inches of nitrous air part with 3,5 gr. of phlogiston when they communicate their phlogiston to as much common air as will take it up. I say, that nitrous air *parts* with *as much* phlogiston, because it is certain, that it does not part with the *whole* of its phlogiston to common or dephlogisticated air, for it contains much more, as already shewn, and, as appears by the red colour, it constantly assumes when mixed with common or dephlogisticated air, which colour belongs to the nitrous acid combined with its remaining phlogiston, and not to the fixed air then produced, nor to the phlogisticated air remaining, as is very evident. Hence the acid, thus formed, is volatile.

4 PR. 267.

One measure of the purest dephlogisticated air and two of nitrous air occupy but  $\frac{3}{4}$ th parts of one measure, as Dr. PRIESTLEY has observed, vol. IV. p. 245. Suppose one measure to contain 100 cubic inches, then the whole very nearly of the nitrous air will disappear, its acid uniting to the water over which the experiment is made, and 97 cubic inches of the dephlogisticated air, which is converted into fixed air by its union with the phlogiston of the nitrous air; therefore 97 cubic inches of dephlogisticated air take up all the phlogiston which 200 cubic inches of nitrous air will part with; and this we have



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have found to be seven grains; therefore, a weight of fixed air equal to that of 97 cubic inches of dephlogisticated air and 7 of phlogiston, will contain 7 gr. of phlogiston. Now, 97 cubic inches of dephlogisticated air weigh 40.74 gr.; to which, adding 7 gr. we have the whole weight of the fixed air equal 47.74 gr. = 83,755 cubic inches; and consequently 100 cubic inches of fixed air contain 8,357 gr. of phlogiston, and the remainder elementary air.

100 gr. of fixed air contain 14,661 of phlogiston and 85,339 of elementary air; which, when stripped of phlogiston, and impregnated with its proper proportion of elementary fire, becomes again dephlogisticated air. Hence also, 100 cubic inches of dephlogisticated air are converted into fixed air by 7,2165 gr. of phlogiston, and will be then reduced to the bulk of 86,34 cubic inches.

And reciprocally, 100 cubic inches of fixed air, being decomposed, will afford 115,821 cubic inches of dephlogisticated air, and part with 7,2165 gr. of phlogiston, supposing the decomposition to be complete; that is, the dephlogisticated air absolutely pure.

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Having read the foregoing account of the nature of fixed air to Dr. PRIESTLEY, I had the satisfaction to find it met with his entire approbation, which he authorized me to mention, notwithstanding what he had advanced to the contrary in his last publication.

## OF THE QUANTITY OF PHLOGISTON IN VITRIOLIC AIR.

The method I pursued was this :

1st, I found the quantity of nitrous air a given weight of copper afforded when dissolved in the dephlogisticated nitrous acid, and by that means how much phlogiston it parts with.

2dly, I found the quantity of copper which a given quantity of the dephlogisticated vitriolic acid could dissolve; and observed, that it could not dissolve the greatest quantity of copper without dephlogisticating a further quantity which it does not dissolve.

3dly, I found how much it dephlogisticates what it thoroughly dissolves, and how much it dephlogisticates what it barely calcines.

4thly, How much inflammable air a given quantity of copper affords when dissolved in the vitriolic acid to the greatest advantage.

5thly, I deduct from the whole quantity of phlogiston expelled by the vitriolic acid the quantity of it contained in the inflammable air; the remainder shews the quantity of it contained in the vitriolic air.

The particulars were as follows :

1st, 100 gr. of copper dissolved in the dephlogisticated nitrous acid afforded me 67,5 cubic inches of nitrous air, which, according to the before mentioned calculation, contain 4,52 gr. of phlogiston.

2dly, 100 gr. of real vitriolic acid take up or dissolve 54,73 of copper, and 100 gr. of copper require about 182,714 gr. of

of real vitriolic acid to dissolve them. Again, 100 gr. of copper, when dissolved in the vitriolic acid, retain only as much phlogiston as is contained in three cubic inches of nitrous air, that is, 0,2 of a grain; therefore, since 100 gr. of copper give out 4,52 of phlogiston, the vitriolic acid strips it of 4,52 - 0,2, that is, 4,32 gr. of phlogiston.

3dly, To dissolve 70 gr. of copper in the vitriolic acid, to the greatest advantage, 20 more must be slightly dephlogistified; therefore, to dissolve 100 gr. of copper in this acid, 28,6 more must be slightly dephlogistified. 8 grs. of this slightly dephlogistified calx afforded 4 cubic inches of nitrous air; therefore, 28,6 would afford 14,3, which contain 0,958 gr. of phlogiston; but 28,6 gr. of copper, before any dephlogistification, contain 1,292 gr. of phlogiston; therefore, they lose by this slight dephlogistification 0,344 of a grain of phlogiston. Hence, when 100 gr. of copper are dissolved in the vitriolic acid, the quantity of phlogiston expelled is  $4,32 + 0,34 = 4,66$  gr.

4thly, The quantity of inflammable air afforded by the most advantageous solution of 100 gr. of copper in the vitriolic acid is 11 cubic inches, which amount to 0,385 of a grain of phlogiston

5thly, The solution of 100 gr. of copper in the vitriolic acid afforded over mercury 75,71 cubic inches of air; but of this only 11 cubic inches were inflammable air, the remainder therefore was vitriolic acid air, amounting to 64,71 cubic inches.

6thly, Then the whole quantity of phlogiston expelled during the solution of 100 gr. of copper in the vitriolic acid is 4,66 gr.; of this inflammable air contains but 0,385 of a grain: the remainder therefore, which consists of 4,275 gr. must be con-

ained in the 64,71 cubic inches of vitriolic air: therefore, 100 cubic inches of vitriolic air contain 6,6 gr. of phlogiston, and 71,2 gr. of acid, and 100 cubic inches of this air weighing 77,8 gr. 100 gr. of this air contain 8,48 gr. of phlogiston and 91,52 of acid.

## OF THE QUANTITY OF PHLOGISTON IN SULPHUR.

This I endeavoured to find by estimating the quantity of fixed air produced during its combustion.

To the top of a glass bell, which was open, I firmly tied and cemented a large bladder, destined to receive the air expanded by combustion, a quantity of which generally escapes when this precaution is not used. Under this bell, which contained about 3000 cubic inches of air, I placed a candle of sulphur, weighing 347 gr.; its wick (which was not consumed) weighed half a grain: it was supported by a very thin concave plate of tin, to prevent the sulphur from flowing over during the combustion, and both were supported by an iron wire, fixed on a shelf in a tub of water. As soon as the sulphur was fired with a very feeble flame, it was covered with the bell, the air being squeezed out of the bladder. The inside of the bell was soon filled with white fumes, so that the flame could not be seen. In an hour after, the fumes thoroughly subsided, and all was cold. The water rose within the bell to a height equal to 87,2 cubic inches; whence I deduce that 87,2 cubic inches of fixed air were produced, which contain 7,287 gr. of phlogiston, which separated from the vitriolic acid, and united to the dephlogisticated part of the common air under the bell.

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The candle of sulphur being weighed, was found to have lost 20,75 gr.; therefore, 20,75 gr. of sulphur contain 7,287 gr. of phlogiston, besides the quantity of phlogiston which remained in the vitriolic air. This air must have amounted to  $20,75 - 7,287 = 13,463$  gr. which contain 1,141 gr. of phlogiston; therefore, the whole quantity of phlogiston in 20,75 gr. of sulphur is 8,428 gr.; therefore, 100 gr. of sulphur contain 40,61 gr. of phlogiston and 59,39 of vitriolic acid.

Several attempts have hitherto been made to determine the proportion of the constituent parts of sulphur; but all were evidently defective. The first was that of STAHL, who calculated the quantity of phlogiston from that of the acid remaining after slow combustion; but as much, both of acid and phlogiston, was dissipated, and as the remaining acid was also phlogisticated, and attracted much of the moisture of the air, no conclusion whatever could be drawn from this experiment. The second method was, to form a liver of sulphur, and convert this by a gentle long continued heat into a tartar vitriolate, and then calculate the weight a given quantity of alkali would gain by this operation. This was also devised by STAHL, and followed by BRANDT and NEWMAN, and by it they determined the proportion of phlogiston to that of acid to be nearly as 1 to 16. But during the formation of the liver of sulphur, whether in the moist or dry way, much of the phlogiston and acid is dissipated, as is evident by the vapour and smell that proceed from it, their alkali also contained fixed air, which it lost during the operation, and of which they kept no account, as they were ignorant of its existence; and the tartar vitriol formed by them or sal polycreste retained much undecomposed sulphur, as always happens when it is not strongly heated; so that this method also was very imperfect, however some subsequent

sequent chemists who made the experiment with more care concluded from it, that sulphur contained one-seventh of phlogiston. EXLEBEN, § 760.

By weighing flowers of sulphur in a perforated brass box in water, I found its specific gravity to be 1,924. It remained in the water a quarter of an hour before any air issued from it, and then some bubbles arose; but when I opened the box, I found the middle part of the flowers quite dry, so that I make no doubt but some air still remained, and that its specific gravity is still greater. Mr. PETIT weighed it in oil, and found its specific gravity 2,344, which I believe to be nearly the truth.

## OF THE QUANTITY OF PHLOGISTON IN MARINE ACID AIR.

8 gr. of copper dissolved in colourless spirit of salt afforded but 4,9 cubic inches of air, when the air was received over water, and this air was inflammable.

8,5 gr. of copper being dissolved in the same quantity of the same spirit of salt, and the air received over mercury, afforded 91,28 cubic inches of air; but of these only 4,9 cubic inches were inflammable air; the remainder, therefore, *viz.* 86,38 were marine air, which weigh 56,49 gr.

Now, as spirit of salt certainly does not dephlogistificate copper more than the vitriolic acid does, it follows, that these 4,9 cubic inches of inflammable air, and 86,38 cubic inches of marine air, do not contain more phlogiston than would be separated from the same quantity of copper by the vitriolic acid: and since 100 grains of copper would yield to the vitriolic

H h 2

acid

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acid 4,32 gr. of phlogiston, 8,5 gr. of copper would yield 0,367 of a grain of phlogiston; this then is the whole quantity extracted by the marine acid, and contained in 91,28 cubic inches of air, and deducting from this the quantity of phlogiston contained in 4,9 cubic inches of inflammable air ( $=0,171$  of a grain), the remainder, *viz.*  $0,367 - 0,171 = 0,196$  is all the phlogiston that can be found in 86,38 cubic inches of marine air. Then 100 cubic inches of marine air can contain but 0,227 nearly of a grain of phlogiston 65,173 of acid.

Hence we see why it acts so feebly on oils, spirit of wine, &c. having a very small affinity to phlogiston; and why it is not dislodged from any basis by uniting with phlogiston, as the vitriolic and nitrous acids are, its affinity to it being inconsiderable.

XVI. *Del modo di render sensibilissima la più debole Elettricità sia Naturale, sia Artificiale. By Mr. Alexander Volta, Professor of Experimental Philosophy in Como, &c. &c.; communicated by the Right Hon. George Earl Cowper, F. R. S.*

Read March 14, 1782.

1. **U**N apparecchio, che portando a uno straordinario ingrandimento i segni elettrici fa sì, che osservabile divenga e cospicua quella virtù, che altrimenti per l'estrema sua debolezza sfuggirebbe i nostri sensi, ognun comprende di quale e quanto vantaggio sia per riuscire nelle ricerche sull' elettricità, e massime intorno alla naturale atmosferica, la quale, come sappiamo, non in ogni tempo, anzi assai di rado, allora solamente cioè che il cielo è ingombro di nuvoloni scuri e tempestosi, avviene che ci renda sensibile ne' conduttori ordinari non molto elevati, e appena è che in altri tempi ne mostri qualche indizio in quelli elevatissimi, o ne' *cervi volanti* portati all' altezza di più' centinaia di braccia. Or un tale apparecchio, mercè di cui un conduttore atmosferico, anche di non grande elevazione, vi dia segni ad ogn' ora e in ogni costituzione di tempo, molto chiari e distinti di quel qualsivoglia picciolo elettrizzamento che in lui induce l'atmosfera, ecco io ve lo presento nel mio elettroforo: in quella semplice macchina, che è ormai nelle mani di tutti, e che se altro pregio pur non avesse verrebbe abbastanza raccomandata agl' Elettrici per questo che lor offre facile mezzo di spiare la più languida e impercettibile elettricità sì naturale che artificiale, con tirarla sopra di sè, ed accumularla



accumularla al punto di promoverne e invigorirne per singolar maniera i segni.

2. In vero ogni volta che questi mancano nell' ordinario modo di sperimentare, e ne scintilla scorgesi nè cenno benché minimo di attramento, il dire che pur vi sia elettricità, fora un' asserzione gratuita, anzi un giudicare contro ogni apparenza. Malgrado questo non possiamo neppur dire accertatamente che punto non ve n' abbia: e il concluderlo da ciò solo che niun segno per anco ci si mostra, è un precipitare il giudizio; imperocchè chi ci assicura che qualche elettricità ivi non si truovi realmente, ma così debole da non poter attrarre tampoco un legger filo? Or questo è che c'importa in molti casi di sapere, specialmente quando si tratta di elettricità naturale. Un conduttore atmosferico poco elevato non dà ordinariamente segni, come già si è detto, che quando gli sovrasta oscuro nembo: a cielo coperto d'alte nubi sparse o distese equabilmente, quando l'aria è ingombrata da nebbie, in tempo di pioggia placida ed anche dirotta, tranne qualche rovescio improvviso, raro è che scorger vi si possa alcun indizio di elettricità, o nulla mai a ciel sereno, sia placido, sia ventoso. Stando pertanto alle apparenze, e al giudizio di un elettroscopio comune, anche dè più sensibili, direbbesi che il conduttore non è elettrizzato punto, e che per conseguenza non domina elettricità di sorta ne' campi dell' aria poco alti ove quel conduttore porta la testa. Eppure non è così: un altro elettroscopio di gran lunga migliore qual veramente può dirsi il nostro apparecchio, giacchè ne adempie con tanto vantaggio le funzioni, ci fa vedere che da qualche elettricità è pur sempre investito quel conduttore, avvegnache ne si mostri di per sé affatto inerte: ci fa, dico, vedere e toccar con mano ch' egli non ne è mai privo affatto; onde convien giudicare in egual modo che non ne è mai priva l'aria che lo circonda. Ed ecco come  
restiamo

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restiamo convinti che anche alla più bassa regione dell' atmosfera, e fino a pochi piedi da terra s'estende l'azion costante e perenne dell' elettricità naturale. Cotal elettricità sebbene insensibile rimanga finche da quel tratto d'atmosfera si comunica soltanto al detto conduttore; ove poi per mezzo di lui si comunichi insieme all' elettroforo nostro, si raccorà entro a questo più facilmente, e in maggiore copia\*; si è per tal modo, che forger quindi potranno i noti segni di attrazione e di ripulsione sensibili abbastanza per dinotarci senza equivoco non che l'esistenza, la specie ancora dell' elettricità, cioè se *positiva* o *negativa*. Che più? non mancherà talora di comparire per fino qualche scintilluzza. Ogniqualvolta poi il conduttore desse già di per se qualche segno, movendo alcun poco un legger filo, aspettatevi pure col' soccorso del' nostro apparecchio, scintille pungenti, e ogn' altro segno vigorosissimo.

3. Mà veniamo senza più al modo di far servire all' intento cotal apparecchio, a cui in questo caso meglio che il nome che altronde porta di elettroforo, l'altro già indicato di *elettroscopio*, anzi pure quello di micro-elettroscopio potrebbe convenire. Ma io amo meglio di chiamarlo *condensatore* dell' elettricità, per usare un termine semplice e piano, e che esprime a un tempo la ragione e il modo de' fenomeni di cui si tratta, come vedrassi nella 2ª parte del presente scritto. Tutto dunque si riduce a queste poche operazioni.

(A) Convien prendere un piatto d'elettroforo, che abbia l'incrostatura di resina assai sottile, e a cui o non sia stata dianzi, impressa alcuna elettricità, o se mai vi è stata, vi si sia spenta, affatto.

(B) A questa faccia resinosa immune da ogni elettricità si soprapponga convenientemente il suo scudo (così io chiamo la

\* Come ciò segua si spiegherà nella 2ª parte di questa memoria.

lamina superiore dell' elettroforo) : voglio dire le si applichi a combaciamento, e si collochi nel bel mezzo in modo, che non tocchi in alcun punto l'orlo metallico del piatto, ma rimanga isolato.

(C) Così congiunti essendo si adattino sotto al filo conduttore dell' elettricità atmosferica in guisa, che lo scudo venga toccato dove che sia dal detto filo, egli solo lo scudo, e in niun modo il piatto.

(D) In questa situazione si lascino le cose per un certo tempo, finché lo scudo possa aver raccolta competente dose di quell' elettricità, che dal filo conduttore gli s'istilla lentissimamente.

(E) Da ultimo sottraggasi al contatto e influsso del filo conduttore lo scudo tuttavia unito al suo piatto; indi si disgiunga anche da questo, levandolo in alto al consueto modo per il suo manico isolante : e allora farà che se ne otterranno gl' aspettati segni cospicui di attrazione, di ripulsione, e di qualche scintilla eziandio, di pennoncelli &c. nel tempo che il conduttore di per sé non giugna a mostrar nulla, o appena un' ombra di elettricità.

4. Ho detto (prec. D) che il filo conduttore debbe toccare lo scudo per un certo tempo. Quanto però non è facile il determinarlo, dipendendo dalle circostanze. Talora vi abbisogneranno 8. 10. e più minuti; quando cioè il conduttore da par sé solo non fa vedere il minimo segno di elettricità : altre volte più poco. Che se un debole indizio pur vi comparisse, tantoche un legger filo facesse cenno d'esservi attratto, basteria in tal caso lasciar in contatto di esso conduttore il nostro scudo sol pochi secondi, per abilitar questo a dar segni molto vivaci.

5. Una cosa si vuol osservare rispetto al filo conduttore medesimo, ed è ch' egli sia ben continuo, o se è possibile d'un pezzo solo dall' alto fino al basso, dove viene a comunicare collo scudo : cioè si dee evitare assolutamente ogni interruzione, e il più

più che si può ancora la semplici giunture ad anello od uncino, per la ragione che ciascuna di tali giunture portando un qualche impedimento al passaggio dell' elettricità, avvenir può che quella, che contrae il conduttore in alto, s'arresti, nè giunga al luogo desiderato, cioè fino allo scudo. Così succederà diffatti ogni qualvolta l' elettricità è debolissima, se in luogo d'un filo metallico continuo, una catena di più anelli da quello pendente venga a toccare cotesto scudo. Non si creda per questo che una sola giuntura o due possano egualmente impedire la riuscita; ma ne verrà sempre del pregiudizio: e qualora l' elettricità fosse estremamente debole, potrebbe sì per l' indicato difetto mancare del tutto l' esperimento.

6. Riguardo all' elettroforo da adoperarsi altre osservazioni rimangono, di cui ora mi convien parlare. E la prima accennata sopra al § 3. (let. A) si è che lo strato resinoso importa molto che sia sottile, avendo io sempre provato che quanto più lo è tanto maggior dose di elettricità permette, anzi fa che si raccolga entro allo scudo cui porta indosso, di quell' elettricità, dico che gli s'infonde o dall' atmosfera per mezzo del filo conduttore, o da qualsivoglia altra potenza elettrica. Se fosse pertanto stesa la resina alla spessezza d'un quarto di linea, o non maggiore di una mano di varnice, riuscirebber le prove ottimamente; siccome all' incontro essendo grossa un pollice o più, andrebber le cose malissimo.

7. In secondo luogo la superficie di essa resina debb' essere quanto si può piana e liscia, e piana e liscia similmente l' inferior faccia dello scudo, sicchè vengano a combaciarsi bene (ivi let. B). E noto quanto un miglior combaciamento favorisca gli effetti dell' elettroforo; ond' ebbi ben ragione di raccomandar questa come una delle principali condizioni nella descrizione che pub-

blicai a suo tempo di questa machina\*. Ma è ancor più grande l'influenza, che l'ampio e perfetto contatto ha sopra l'istesso apparecchio, allorchè il medesimo agisce in qualità di condensatore.

8. Da ultimo merita particolar attenzione quanto alla già citata let. A si è prescritto, che alla faccia resinosa, cui si applica lo scudo, non dee essere impressa alcuna elettricità. La ragione per cui vuolsi che ne sia affatto priva ella è, che altrimenti i segni dello scudo, allorchè s'alza, diverrebbero equivoci, non essendo più la sola elettricità trasfusa in esso scudo dal conduttore atmosferico quella che giuoca, ma insieme anche l'altra occasionata dall' elettricità impressa ed inerente alla faccia resinosa: quando a noi importa di esplorare la sola sopravveniente la detto scudo.

Se dunque la faccia resinosa del piatto, di cui volete servirvi, è rimasta sempre intatta, va bene. Ma se è stata già eccitata, e vi si mantiene tuttavia parte dell' impressa elettricità, egli conviene fare di tutto per ispegnarla; cio che non è sì agevol cosa. Il passarvi sopra un panno alquanto umido, applicandolo ben bene a tutta la superficie, è un de mezzi più efficaci ch' io mai abbia trovato †; pur non toglie talvolta che dopo qualche tempo lo scudo posatovi sopra, e previo il solito toccamento, rialzato, non attragga sensibilmente un filo. Lo stesso succede non di raro anche dopo aver tuffato tutto il piatto nell' acqua, lasciatovelo un pezzo, e quindi fattolo rasciugare all' aria, Lo squagliare la superficie della resina al fuoco o al sole, è forse il più sicuro spediente per farne svanire tutta quanta l'elettricità,

\* Si truova questa descrizione in un colle principali esperienze, e un piccol saggio di spiegazione, in due memorie indirizzate in forma di lettera al Dr. PRIESTLEY, e pubblicate in un' opera periodica di Milano intitolata *Scelta d' Opuscoli interessanti*, per l'anno 1775.

† Vegg. l'accennata descrizione dell' elettroforo.

sicchè non ne rimanga pur ombra o vestigio nella stessa resina, rassodata che sia \*. Una maniera più spedita è di far passare sopra tutta la faccia della resina la fiamma di una candela, o d'un foglio di carta acceso. A qualunque però di tai mezzi uno si appigli, per accertarsi che l'elettricità sia spenta a segno che più non possa aver parte alcuna l'azione propria dell' elettroforo agli effetti che risultar debbono unicamente dall' elettricità infusa allo scudo dal conduttore atmosferico, converrà far prima la prova se posato esso scudo sulla faccia resinosa, toccato col dito, e rialzato al consueto modo, non mova neppure un sottilissimo pelo: allora non producendo alcun effetto in qualità d'elettroforo, servirà ottimamente all' altr' uso, cui vien destinato, di condensatore dell' elettricità.

9. Se mi si dimandasse ora a qual grado giunga nel descritto apparecchio cotal condensazione dell' elettricità, cioè a quanto maggior forza forger possano i segni elettrici nello scudo quando s' alza, risponderei che non è facile il determinarlo, dipendendo da molte circostanze. Che però, le altre cose pari, l'aumento è maggiore in ragione che il corpo che fornisce l'elettricità allo scudo si truova avere maggiore capacità; ed è più grande in proporzione che la forza elettrica im-

\* È stato creduto per molto tempo che il calore, e molto più la liquefazione del solfo e delle resine, bastasse senz' altro ad eccitarvi l'elettricità. Ma tranne la tormalina, ed alcune altre pietre, che sì veramente concepiscono l'elettricità pel solo calore, le resine e il solfo non è mai che lo facciano, se loro non sopravvenga qualche stropicciamento, o tocco almeno d'altro corpo. L'errore è nato, come ha avvertito il Prof. BECCARIA con altri, da che ogni legger tocco della mano, o di checchè altro puo bastare in tali circostanze favorevoli. Senza questo la materia fusa abbandonata a se stessa nel rapprenderfi e dopo, tanto è lungi che contragga alcuna elettricità, che anzi perde quella qualunque che per forte aver potesse prima della fusione, come le nostre sperienze ci assicurano. Nè sia meraviglia: giacchè tutti i corpi coibenti per un forte grado di calore divengono conduttori; e i corpi resinosi in ispecie lo sono già quando si trovan molto rammolliti, e più allorché cominciano ad entrare in fusione.

piegata è più debole. Così vedemmo giù che se il conduttore atmosferico non ha la forza di alzare d'un grado il pendolino dall' elettrometro, movendo appena un sottil pelo, ed anche meno di questo, potrà tuttavia abilitare lo scudo non che a vibrar l'elettrometro a più gradi alto, ma a scagliare pur anche vivace scintilla (§ 2. e seq.). Ma se l'elettricità nel conduttore atmosferico farà più forte a segno di dare qualche scintilletta, di mandare l'elettrometro a 50,6 gradi, lo scudo che riceverà questa elettricità, darà gli è vero una scintilla affai più forte, e l'elettrometro vibrerà al più alto punto es. gr. a 100 . 120 gradi. Ad ogni modo è visibile che la condensazione dell' elettricità ne in questo é minore che nel p.<sup>o</sup> caso, in cui venne aumentata sì, ma non di 60 volte, la ragione è che al di là del massimo non si può andare, cioè di quel grado a cui giunta l'elettricità si dissipa da se stessa aprendosi il passaggio per tutto. Dunque a misura che la potenza elettrica, la quale si applica allo scudo posato, è più vicina a tal sommo grado, minor accrescimento può ricevere dall' apparecchio condensatore. Ma che bisogno abbiamo noi allora di lui, e tutte le volte che l'elettricità è già sensibile e forte abbastanza?

L'uso a cui vien destinato è di sottrarre, e raccolta sopra di se sufficiente dose render sensibile quella, che è languida affatto e impercettibile, finchè rimane nel gran conduttore in pace (1.).

10. Quando dunque il conduttore vi dà già da se solo segni abbastanza distinti di elettricità, non accade ricorrere all' altro apparecchio. Dirò dippiù che il farlo può produrre un grande inconveniente, ed è, che per poco che l'elettricità del conduttore sia vigorosa, a segno di dare qualche scintilla, avviene allora che facendogli toccare lo scudo, l'elettricità non si arresti in lui solo, ma che passi in parte ad imprimerfi alla faccia resistosa cui copre; onde in seguito l'apparecchio prenda a fare le

funzioni di vero elettroforo: ciò che per le ragioni già dette (8.) si dee con ogni studio evitare.

11. Per prevenire un tal inconveniente ho pensato di surrogare al piatto incrostato di resina, un piano che non fosse vero e perfetto isolante, assolutamente impermeabile al fluido elettrico; ma tale solamente che opponesse una discreta resistenza al suo passaggio, come una lastra di marmo asciutta e politissima, un piattello di legno similmente asciutto ed arido, oppure incrostato di gesso, o meglio ancora inverniciato, una tela incestrata secca e monda, od altro simile. Alla superficie di tali corpi non avverrà d'ordinario che s'affigga l'elettricità potendo appiccata che sia scorrere e trapassare per entro ad essi; o se pur tal volta ve ne rimanesse un pocolino, quasi stagnante, sia questa passeggera, in brevi momenti svanita. Quindi è che un tal apparecchio inatto alle funzioni d'elettroforo non ce ne darà i fenomeni; ma per questo appunto meglio servirà all' altr' uso di condensatore.

12. Sostituendo così allo strato resinoso o a qualsivoglia altro coibente perfetto un piano o strato che sia mezzo tra coibente e deferente, cioè un corpo isolante molto imperfetto e insieme imperfettissimo conduttore, quali sono nelle divise circostanze gl' indicati corpi (prec.) non solamente si toglie o si fa minore il pericolo di qualche elettricità che possa imprimerfi e restar aderente alla superficie del piatto, la quale renderebbe equivocate le sperienze delicate; ma inoltre un notabile vantaggio da noi si ottiene, ed è che lo scudo posato su tai piani non affatto isolanti cava del conduttore, e si tira addosso maggior dose di elettricità, che se posato fosse sopra uno strato resinoso, od altro perfetto coibente. E come detto già abbiamo (6.) che uno strato resinoso quanto è men grosso, tanto più abilita la lamina che gli è sovrapposta ad arricchirsi di elettricità; così tale strato  
ridotto



ridotto ad una semplice vernice, o intonaco di cera, l'una e l'altra già men coibente della resina, e infine ridotto a niente, sostituendovi soltanto una superficie poco deferente, come quella del marmo o del legno arido, somministra alla lamina metallica la più favorevole positura che mai aver possa per raccogliere nel suo seno abbondante elettricità.

13. Guardiamoci però nel voler ischivare il troppo di coibenza di dare nel poco, accostandoci ai deferenti perfetti, o quasi perfetti. Non bisogna perder di vista, che la superficie del piatto dee opporre una discreta resistenza al trapasso del fluido elettrico, per rattenere una competente dose di elettricità nello scudo addossatole (11.). Ma basta che ciò faccia per un qualche picciolissimo tempo; d'uopo essendo non rare volte di tenervi confinata l'elettricità otto, dieci, e più minuti, quanti cioè ne impiega il conduttore atmosferico a raccogliere dall'aria, ed infondere in esso scudo tal copia di elettricità, che possa rendersi sensibile e cospicua (3. D. 4.).

Dal che facilmente s'intende quanta attenzione pur convenga e nella scelta del corpo da surrogarsi allo strato di resina, e nella convenevole preparazione del medesimo: la quale preparazione consiste generalmente in certo grado di essiccamento, che lo riduca allo stato di semicoibente nè più, nè meno. Ad ogni modo fia meglio peccare per eccesso di coibenza, che per difetto; meglio prendere un piattó qualsivoglia incrostato di resina, che un desco di legno nudo non aridissimo, una lastra d'osso, od una di marmo comune non previamente riscaldato al sole o al fuoco: giacchè niun osso, e pochissimi tra i marmi ho trovato che valgano a tener confinata l'elettricità nello scudo che combaciano oltre ad un minuto o due al più, se abilitati non vengano da un convenevole riscaldamento. Disposti però che siano in tal modo, e ove singolarmente incontrata si sia ottima qualità nel

nel marmo, riescono a meraviglia, e sorpassano ogni aspettazione; onde sofferirò sempre con ragione, che si fatti piani di legno, d'osso, di pietra, nudi come sono, e ancora notabilmente deferenti, meritano tuttavia d'essere preferiti a un ordinario piatto d'elettroforo fornito del suo strato resinoso.

14. Venendo ora più davvicino alla maniera, onde praticamente si può ridurre il nostro apparecchio alla maggior perfezione, per ritrarne il più gran vantaggio, dopo aver ricordato come conviene soprattutto che lo scudo s'adatti bene a combaciamento col piano sottoposto (3 lat. B. e 7.), soggiungerò che per ottener ciò nel miglior modo è bene d'appigliarsi ad una lastra di marmo, e questa insieme alla lamina o scudo metallico spianare ben bene, lavorandole una sopra l'altra, finchè sian ridotte a tale perfetto combaciamento, che ne nasca sensibile coesione tre loro.

Il marmo poi così lavorato si esponga per molti giorni al calore d'una stufa, con che espellendosi l'umido di cui anche tali pietre sono spesso inbevute, verrà il marmo condotto a questo stato d'imperfettissimo conduttore, che è l'ottimo per le sperienze di questo genere (12. 13.); e si manterrà tale per un pezzo, sol che non resti lungamente esposto al grand' umido: poichè per quell' umidore che può contrarre accidentalmente, e in poco tempo, non essendo che superficiale, non verrà esso marmo a deteriorarsi notabilmente; e basterà prima di sperimentare esporlo per alcuni minuti al sole, o pur anche asciugarlo con un pannolino caldo.

15. E qui giova avvertir di nuovo, che non tutti i marmi sono egualmente buoni. In generale i più vecchi, e che da molto tempo sono stati guardati dal grand' umido riescono incomparabilmente meglio che quelli tratti di fresco dalla cava, o stati esposti lungamente all' ingiurie dell' aria; onde quest  
solamente

solamente han bisogno dell' efficcamento nella stufa. Ma oltre di ciò avvi ancora notabilissima differenza tra una specie e l'altra di marmo: ne ho trovato di tali, che senza riscaldarli ne tampoco asciugarli fanno sempre a meraviglia, e di tali altri, che anche con una tal preparazione non corrispondono troppo bene; a meno che non si continui loro il' caldo durante il tempo dell' esperienze. Sopra tutti finora ho trovato eccellente il bel marmo bianco di Carrara. Ciò non pertanto io non so abbastanza raccomandare di riscaldare e questo, e gl' altri marmi, almeno un poco innanzi adoperarli: con che vantaggian sempre per eccellenti che siano ed essendo cattivi vengono a migliorarsi insignemente, e si adagguagliarsi ai più buoni: anzi posso dire, per esperienza che la maggior parte dei marmi di lor natura poco buoni, ove siano ben riscaldati previamente, e in seguito si mantengano tiepidi tutto il tempo dell' esperienza, prevalgono se non a tutti a molti dei migliori non punto riscaldati.

16. A chi però sembrasse incomoda questa preparazione (la quale per altro a che si riduce? Ad esporre il piatto di marmo al sole, od a presentarlo per poco d'ore innanzi al fuoco d'un cammino, o al più tenerlo su d' uno scaldavivande ove sia o cener calda o pochi carboni accesi) io suggerirò il mezzo di dispensarsene: basta di dare alla faccia piana del marmo una buona mano di vernice copal, da asciugarli quindi in una stufa ben calda o in un forno tantoche prenda un color d'ambra tirante al bruno. La vernice medesima d'ambra farà ottima, siccome pure la lacca. Con ciò non solo i buoni, ma i cattivi marmi eziandio serviranno mirabilmente all' intento (chè si pure un gran vantaggio) e serviranno in ogni tempo senza previo riscaldamento, o almeno senza continuarlo loro durante l'esperienza.

17. Appigliandosi a questo spediente della vernice si può benissimo in luogo del piatto di marmo far servire una lamina di metallo eguale all' altra lamina o sia scudo, e resa perfettamente combaciante col lavorare, come si è detto (14.), i due piani un sopra l'altro. Se la vernice si desse ad amendue le faccie combacianti, non farebbe male; ma basterà anche il darla all' una o all' altra: in questo caso però una mano sola di vernice; che farebbe più che sufficiente, per la lastra di marmo, forse non basterebbe per la lamina metallica, ma ce ne vorrebbe una seconda ed anche una terza mano.

18. Ma con ciò, mi si dirà, noi siam ricondotti ad un vero piatto d'elettroforo, giacchè l'intonaco di vernice tien qui luogo del sottile strato di resina. Io non voglio negarlo; anzi dirò, d'aver provato che e il metallo, e il marmo, singolarmente così inverniciati, son tali, che l'elettricità vi si affigge facilmente, e non men facilmente vi si eccita per istrofinamento, talchè il solo strisciare che faccia lo scudo sulla superficie inverniciata del piatto, o il percuoterla con qualche forza mentre si viene a posar sopra cotesto scudo, basta perchè poi dia segni sensibili di elettricità allorchè si distacca. Talora anzi non è possibile d'impedire che questo succeda, per quanto si procuri di posar lo scudo pian piano, e di alzarlo senza punto strofinare. Tal importuna elettricità però è debolissima e non si suscita che nel caso in cui il piatto verniciato si trova asciugatissimo, e ancor tiepido dal sole o dal fuoco. Si fatto asciugamento e riscaldamento adunque non solo non è necessario per le nostre sperienze quando adoperiamo un piano verniciato, com'è necessario quasi sempre ove s'adoperi il marmo nudo (13. 15. 16.), ma non è neppure molto proficuo da una parte; e dall' altra egli è assolutamente pregiudizievole, per ciò che dando luogo ai fenomeni d'elettroforo, può facilmente produrre equivoci ed incertezze (8.).

19. Qual vantaggio adunque, mi si dirà un' altra volta, nell' adoperare in luogo di un ordinario elettroforo, un piatto inverniciato? Altronde si è pur detto che vuol preferirsi un piatto nudo di marmo (11. e seq.). Il vantaggio del piatto verniciato sopra l' un ordinario d'elettroforo è 1° che la vernice farà sempre più sottile di qualunque incrostatura resinosa; 2° che quella meglio che questa può lasciare la superficie del piatto, sia di marmo sia di metallo, piana e liscia in modo, che lo scudo vi s'adatti ancora quasi a coesione: due circostanze, quali veduto già abbiamo (6. 7. 14.) quanto influiscano alla buona riuscita delle sperienze di cui si tratta. Riguardo al piatto nudo di marmo, egli è ben vero che questo può servire egualmente bene, e forse meglio s'egli è d'ottima qualità, o allorché si tenga convenevolmente riscaldato (15.); ma valutando bene le cose, l'incomodo, cioè di tal preparazione, qualunque egli sia (16.), e la difficoltà d'aver il marmo perfetto (15.), credo che convenga ancora l'espediente della vernice, che vi dispensa da tutto questo (16.). Vi resta è vero l'altro inconveniente di potervisi per poco affiggere l'elettricità; ma oltrecchè anche il marmo perfettamente asciutto, e molto più se caldo, non va esente da tal incomoda disposizione, egli non è poi tanto difficile di ciò scansare adoperando le debite attenzioni; e l'accurato sperimentatore non lascerà di assicurarsi coi mezzi che già si sono indicati (8.) che non trovasi neppur ombra di elettricità impressa alla faccia verniciata, quando imprende a fare col condensatore delle sperienze delicate.

20. Al piatto di marmo e di metallo inverniciato va di parò un piano qualunque coperto di buona tela incerata secca e monda, di taffetà cerato, di raso o d'altro drappo di seta il quale più che è sottile è meglio: dico, che questi piani così vestiti van di parò agl' altri verniciati, stante che non han bisogno che  
 .....

d'avere cotal veste ben asciuta, e al più un pocolino riscaldata prima di servirsene; anzi pure e la tela, e il taffetà, incerati non attraendo molto l'umido, non hanno d'ordinario neppure bisogno d'essere posti al sole o al fuoco innanzi farne uso. Il ciamberlotto, il feltro, ed altri drappi di pelo son buoni anch' essi, ma men della seta; quei di lana, o di cotone, meno ancora; e i più infelici sono quei di canape e di lino: ad ogni modo un buon asciugamento, e un gentil calore continuato possono abilitare anche questi, siccome pure abilitano la carta, il cuojo, il legno, l'avorio, e gl' altri ossi, tutti insomma i corpi che sono da se stessi imperfettissimi conduttori, anzi non conduttori, ma troppo bibaci dell' umido, cui perciò convien espellere fino a un certo segno.

21. Dico *fino a un certo segno*: perchè un troppo grande isolamento è pregiudizievole anzichè no, come si è già accennato (6. 12.), e come si farà più chiaramente vedere nell' 2.<sup>a</sup> parte di questa memoria. Or dunque se i detti corpi vengano spogliati affatto d'umido, posti per esempio a seccare nel forno, in tal caso siccome diverranno veri e perfetti coibenti a par delle refine; così non serviranno più al nostro intento, a men che non sian ridotti ad uno strato sottile, e questo strato applicato ad un conduttore (ivi) in modo che ne risulti un vero piatto d'elettroforo.

22. Non lascerò da ultimo di dire, che si può rendere l'apparecchio ancora più semplice, se si applichi sia l'intonaco di vernice, sia la veste d'incerato, sia il taffetà od altro velo di seta, sia infine qualunque materia semicoibente, alla faccia inferiore dello scudo, in luogo di coprirne il *piatto*; il quale in questo caso diventa inutile, servendo allora in sua vece un piano qualunque egli sia, un tavolo di legno o di marmo, anche non ben asciutti, una lastra di metallo, un libro, od altro conduttore, buono o

cattivo che sia, sol che vi si possa applicare convenientemente la faccia vestita dello scudo.

E' in vero altro più non si ricerca per la buona riuscita delle sperienze, se non che l'elettricità, che tende a passare dall' uno all' altro dei piani combaciantisi, incontri sull' una delle superficie tale resistenza, che valga a trattenerla, come si è già accennato (11), e si farà chiaro nella stessa seconda parte; dove al dippiù mostrerassi, come a tal effetto basti anche una picciola resistenza. Ciò posto, che lo strato sottile coibente o quasi coibente tenga al piano di sotto, o a quel di sopra, egli è lo stesso: quello che importa è che si combacino bene (7); la qual cosa non è sì facile ottenere allorchè si posa lo scudo su d'un tavolo; od altro piano non preparato a bella posta. Egli è solo per questa ragione, per ottenere cioè un più esatto combaciamento, che io dò la preferenza a due piani lavorati un sopra l'altro intonacandoli quindi od amendue, o uno solo, qual più mi piace (14. 17.). Del resto la comodità d'avere per tutto l'apparato una sola lamina di metallo inverniciata da un lato, o coperta di taffetà, e dall' altro guernita di tre cordoncini di seta, fa che io me ne serva più comunemente: e la riuscita se non agguaglia per avventura quella dell' altro apparecchio composto dei due piani lavorati un sopra l'altro, è tale però che basta d'ordinario all' intento.

23. Fin qui noi abbiamo considerato l'utile che si può ritrarre dal nostro apparecchio condensatore applicato ai conduttori per esplorare l'elettricità atmosferica, allorchè è debole affatto ed impercettibile\*. Questo però, a cui vien destinato

\* A questo proposito non debbo omettere, che nè pochi giorni in cui m'applicai a spiare l'elettricità atmosferica col soccorso del condensatore, non son rimasto senza buon frutto raccorre. Il Sig. CANTON, ed altri assicuravano di aver ottenuto dall'

principalmente, non è il solo uso che far se ne possa, nè il solo vantaggio che ci procura: serve altresì molto per l'elettricità artificiale, a scoprirla cioè ove per altra via non si manifesterebbe, o a renderne i segni assai più cospicui. Molti sono i casi, in cui, l'elettricità, che è nulla in apparenza, o molto dubbia, vi si renderà chiara e sensibilissima coll' ajuto di tal apparecchio: ne andrò accennando per modo d'esempio alcuni.

24. 1.<sup>o</sup> Una boccia di Leyden caricata, e quindi addotta alla scarica coll' applicarvi tre, o quattro volte l'arco conduttore, e con replicati toccamenti della mano, vi sembra omai spogliata affatto della sua elettricità. Ma che? Toccate coll' uncino di tal boccetta la lamina metallica posata convenevolmente (cioè sopra qualunque piano, s'ella è ben inverniciata nella faccia inferiore, o vestita di taffetà, &c. oppur s'è nuda sopra sottile strato resinoso, o su d'un incerato, o su drappo di seta, o sopra tavolo di legno inverniciato, o sopra lastra di marmo ben asciutto) e tosto alzata eotal lamina o scudo ne avrete segni elettrici sensibilissimi: dal che concluderete che l'elettricità della boccetta non era già tutta spenta, come appariva. Che se questa avesse una carica sensibile a segno di

dall' apparato atmosferico de' segni elettrici più vivi dell' ordinario in tempo di qualche aurora boreale; ma molti de' fisici non sono persuasi ancora che l'elettricità influisca in queste meteore, e alcuni lo negano formalmente. Io stesso ne dubitai moltissimo: ora però parmi la cosa certa; e posso dire d'aver veduto e toccato con mano. In quella bellissima aurora comparsa nella notte dei 28 ai 29 Luglio dell' anno 1780, quando salendo a poco a poco dall' orizzonte fu ascesa tra la 4. e le 5. italiane allo zenit, spargendo tutt' all' intorno un vaghissimo lume rossigno, il cielo altronde essendo sereno e ventoso, si ottennero col favore dell' apparecchio condensatore da un conduttore atmosferico ordinario molte belle scintille chiare e crepitanti: quando in tutti gl' altri tempi sereni, e in ogni ora del giorno e della notte dall' istesso conduttore, e coll' ajuto dell' istesso cendensatore o non ottienfi scintilla o minutissima soltanto; e ciò perchè quel conduttore atmosferico non è nè alto molto, nè molto ben situato.

attrarre



attrarre un legger filo, in tal caso lo scudo toccato dall' uncino anche per un sol momento, e quindi alzato vibrerà vivace scintilla. Riposto quello, e ritoccato coll' istesso uncino della boccia, e rialzato di nuovo, ne otterrete una seconda scintilla, nulla o poco men vivace della prima; e un tal giuoco potrássì continuare per molte volte con pari diletto e meraviglia.

Cotesto artificio di produr scintilla, e replicate, con una boccetta, che non ha carica sufficiente per farlo da sè sola, vi appresta una grande comodità per varie sperienze dilettevoli, come quelle della mia pistola, e della lucerna ad aria infiammabile, massimamente trovandovi provveduto d'una di quelle boccette preparate alla maniera del Sig. TIBERIO CAVALLO\*, le quali si possono portare cariche in tasca molto tempo. Queste poichè conservano una carica sensibile alcuni giorni, ne conserveranno una insensibile per settimane, e mesi: insensibile, dico, senza l'aiuto del nostro apparecchio condensatore; ma con questo sensibilissima, e più che sufficiente all' uopo di accender la pistola, &c.

25. 2°. Avete una macchina elettrica meschina, così mal in ordine, e in tali circostanze sfavorevoli d'umido &c. che non potete trarre la più piccola scintilla dal conduttore, il quale appena attrae un leggerissimo filo, o non giugne neppur a tanto. Or via fate toccare a tal conduttore inerte il nostro apparecchio, ossia lo scudo posato come conviene, e lasciate che il toccamento duri per qualche minuto, tenendo sempre in azione la macchina, e vi riuscirà di ottenere col solito giuoco di sfaccare lo scudo dal sottoposto piano, una buona scintilla, e ogn' altro segno vivace.

26. 3°. Sia pur la macchina buona, e agisca a dovere; ma il conduttore trovisi così male isolato, che l'elettricità non vi si

\* Vedi il suo trattato di elettricità.

possa

possa accumulare a segno di dar scintilla, e neppure di attrarre un filo: come quando l'istesso conduttore tocca al muro della stanza, o quando una catena pende da esso sopra un tavolo, e fin sopra il pavimento della stanza. In simil caso crederete che l'elettricità per quelle comunicazioni si disperda intieramente, ma cercando più oltre, ricorrendo cioè al condensatore, troverete che un poco se ne trattiene ad ogni momento nel conduttore tuttochè non isolato, e tanto che durando l'azione della macchina qualche tempo, i molti pochi raccolti insieme nello scudo, per la vantaggiosa disposizione ch'egli ha di tirar sopra di se l'elettricità (2.) fanno ch' il medesimo sia poi in istato di dar segni abbastanza forti.

27. 4°. L'ordinaria maniera di strofinare alcuni corpi, e quindi presentarli ad un elettrometro, onde vedere se per tal mezzo abbiano o no contratto qualche elettricità, e in molti casi insufficiente, di modo che sovente si crede che sia nulla, sol perchè debolissima. Si trae dunque un gran vantaggio strofinando corpi dubbi collo scudo o lamina metallica del nostro apparecchio, che in questo caso deve esser nuda, poi levatala in alto isolata interrogando lei medesima, la quale darà segni abbastanza sensibili per qualunque picciola ad insensibile elettricità eccitata nel corpo, contro cui si è strofinata, e dinoterà quale specie di elettricità quello abbia contratta, giacchè si sa che debbe essere nei due contraria. Anche il Sig. CAVALLO si serviva di questo mezzo per iscoprire l'elettricità in molti corpi \*. Ma ve n' è uno a certi riguardi migliore, che certamente nè egli nè altri, ch' io sappia, han conosciuto. Quando il corpo, di cui si vuol provare la virtù, non è tale che vi si possa addattare in piano la lamina metallica per dimenarla sopra strofinando, si può fare così: posata la lamina sopra il solito piano

\* Vedi il suo trattato, cap. VI. p. iv.

femicoibente, si strofini essa, o meglio si percota a vari colpi col corpo in questione; il che fatto si levi la lamina, e si offervi se è elettrizzata: lo farà certo nel caso che vi siate servito a percuoterla di una striscia di cuojo, d'una corda, d'un pezzo di panno, di feltro, o simili cattivi conduttori; e lo farà assai più che se l'aveste sferzata o strofinata par egual maniera coi medesimi corpi stando essa lamina metallica isolata. In somma coll' uno o coll' altro degl' indicati mezzi voi otterrete elettricità da corpi che non avreste mai creduto che godessero di questa virtù, anche da corpi non secchi, da tutti infine eccetto solo i metalli e i carboni: dirò dippiù, ch' io ne ho ottenuto qualche volta strofinando la lamina metallica colla mano nuda.

28. 5°. Si è cercato se il calore, l'evaporazione, le fermentazioni, &c. producano qualche grado di elettricità, ossia cagionino qualche alterazione alla dose naturale del fluido elettrico nei corpi che subiscono cotesta azione, e in quelli che ne sono in contatto. La ricerca era di grande importanza per fissar pure qualche idea sull' origine dell' elettricità naturale, ossia atmosferica. Io so di molti che hanno tentato specialmente sull' evaporazione delle sperienze invano, anche hanno infine rinunciato alla speranza di ottenere per tal mezzo segni elettrici; nè so d'alcuno che sia ancor giunto ad ottenerli. Le mie proprie sperienze non avean avuto miglior successo; ad ogni modo ben lungi di rinunciare ad ogni speranza, le andava sempre più nodrendo. Da gran tempo io aveva immaginato che le dissoluzioni, le effervescenze, le volatilizzazioni, &c. sconvolgendo le minime particelle, e forma e posizione mutandone, doveano coll' alterazione delle forze mutue di esse particelle aumentare o diminuire le rispettive capacità dei corpi sottoposti a que moti intestini, e conseguentemente occasionare dove condensazione, dove rarefazione del fluido elettrico: ne era così persuaso, che  
non

non sapevo darmi pace che l'elettricità non si manifestasse per alcuno di tai processi; di tal mancanza di segni pertanto io ne accagionavo parte alla debolezza dell' elettricità che per tal modo si eccitava, parte alla dissipazione di essa prodotta dai vapori medesimi che si sollevano durante il processo, e distruggono quasi intieramente l'isolamento: mi compiaceva però sempre a pensare, che l'avrei un giorno potuta scoprire cotesta elettricità fugace, moltiplicando le sperienze, e mettendovi più di attenzione e di accuratezza \*. Due anni sono allorchè fui passo passo condotto alla maniera di condensare a un segno sì grande l'elettricità coll' apparecchio qui descritto, i miei pensieri si rivolsero nuovamente all' oggetto delle antiche mie ricerche, e concepj molto più fondata speranza di poter iscoprire qualche cosa, e già mi proponeva di applicarmi, a tali sperienze, quasi presagendo la riuscita; ma varj accidenti le ritardarono fino al Marzo e Aprile di quest' anno, in cui intraprese avendole a Parigi in compagnia di alcuni membri dell' Accademia R. delle Scienze, mi riuscì finalmente di ottenere segni non dubbj di elettricità (che dico segni non dubbj?) fin la scintilla elettrica dall' evaporazione dell' acqua, dalla semplice combustione dei carboni, e da varie effervescenze, come quelle che producono l'aria infiammabile, l'aria fissa e l'aria nitrosa.

29. Terminerò la prima parte di questa memoria coll' dire, che oltre gli accennati vantaggi, ed altri del medesimo genere, che ne procura il nostro condensatore considerato semplicemente come istromento, atto ad ingrandire i segni dell' elettricità; le varie sperienze che possono farfi con esso spargono eziandio molto

\* Tutti questi miei pensieri sono esposti in una dissertazione latina stampata l'anno 1769, che ha per titolo, *De vi attractiva ignis electrici, ac phenomenon inde pendentibus*, ad JOHANNEM BAPTISTAM BECCARIAM, &c.

lume sulla teoria elettrica, per quella parte massimamente che riguarda l'azione delle atmosfere: lo che andiamo a vedere nella parte 2<sup>a</sup>.

## P A R T E S E C O N D A.

*IN qual maniera un conduttore accostandosi a un altro sotto certe condizioni, si truovi in istato di ricevere una straordinaria quantità di elettricità.*

30. Le sperienze riportate nella prima parte di questa memoria ci hanno abbastanza mostrato come una lamina metallica o qualsivoglia piano conduttore, cui foglio appellare *scudo*, applicato ad un altro piano, il quale opponga, o per la qualità sua di cattivo conduttore, o per l'interposizione di un sottile strato coibente, una certa non grande resistenza alla trasfusione dell' elettricità, come dissi, tale scudo in fissata posizione atto sia a tirare sopra di sè e raccorre nel suo seno maggiore copia di elettricità, che se si trovasse in qualsivoglia modo perfettamente isolato. Abbiain veduto come facendolo toccare all' uncino di una boccia di Leyden, al conduttore di una macchina elettrica, o a quello dell' elettricità atmosferica, infine a qualunque potenza o sorgente elettrica, anche quando l'elettricità è debolissima e affatto impercettibile, pur gli se ne comunica tanto da poter manifestarsi quindi con segni molto vivaci, tosto che si leva esso scudo in alto. Or qui intraprendiamo di spiegare un tal fenomeno: e la spiegazione medesima servirà più ch' altra cosa a facilitare la pratica delle sperienze di questo genere.

31. Adunque il tutto si riduce a questo: che la lamina o scudo ha molto e molto maggiore capacità nel 1° caso, quando cioè posa sul piano avente le condizioni indicate (præc. e 11, 12. 22.), che nel 2°, in cui tienfi es. gr. in alto sospeso per i suoi cordoncini di seta, o per un manico isolante, oppur che posa sopra un grosso strato coibente, o sopra un piatto isolato.

Per dilucidare questo punto essenziale, prendiam le cose da più lontano.

32. Non vi vuol molto a comprendere, che' ivi è maggiore capacità, dove una data quantità di elettricità forge a minor intensità, o che è lo stesso, quanto maggior dose di elettricità è richiesta a portare l'azione a un dato grado d'intensità; e *vice-versa*: a dir breve, la capacità e azione, o tensione elettrica sono in ragione inversa.

Farò qui osservare sul principio, ch' io dinoto col termine di tensione (che volentieri sostituisco a quello d'intensità) lo sforzo che fa ciascun punto del corpo elettrizzato per disfarfi della sua elettricità, e comunicarla ad altri corpi: al quale sforzo corrispondono generalmente in energia i segni di attrazione, ripulsione, &c. e particolarmente il grado a cui vien teso l'elettrometro.

33. Ciò che abbiain detto comprenderfi facilmente che la tensione debb' essere in ragione inversa delle capacità, ei viene poi mostrato nella maniera più chiara dall' esperienza. Siano due verghe metalliche, una lunga 1 piede e l'altra 5. di grossezza equali. S'infonda alla prima tanto di elettricità, che giunga a vibrare un elettrometro annesso a 60 gradi: se in questo stato si farà toccare quella all' altra verga, l'elettricità comparendosi equabilmente ad ambedue, diminuirà di tensione tanto appunto, quanto la capacità si truova ora accresciuta, cioè 6 volte: locchè ci farà vedere l'elettrometro, smontando dai 60, ai

L 4 2

10 gradi.

10 gradi. Così se l'istessa quantità di elettricità venisse a diffondersi in un conduttore 60 volte più capace, non rimarrebbe che  $\frac{1}{60}$  della primiera tensione, cioè un grado solo: come *vice-versa* la tensione di 1 sol grado di cotesto gran conduttore, o d'altro qualunque, salirebbe a 60, ove la di lui elettricità venisse a raccorsi e condensarsi in una capacità 60. volte minore.

34. Or non solo conduttori di mole e massa diversi hanno diversa capacità; ma anche l'istesso conduttore può averne una maggiore o minore, secondo varie circostanze; alcune delle quali non sono per anco state considerate, come si conviene. È stato osservato che l'istesso conduttore acquista o perde in capacità, a misura che si aggrandisce, o si restringe di superficie; secondo che una catena metallica es. gr. si dispiega in lungo, o si ammuccia, secondo che vari cilindri contenuti un nell'altro, come quelli d'un cannochiale si traggono fuori, o si fanno rientrare, &c. Quindi si è concluso generalmente che la capacità non è in ragion della massa, ma bene in ragion della superficie del conduttore: come FRANKLIN ha dimostrato appunto coll' indicato sperimento della catena.

35. Questa conclusione è giusta, ma non comprende ancor tutto, perocchè anche con superficie egualmente grandi si ha maggiore o minore capacità, se siano i conduttori diversamente conformati. Essa si troverà maggiore di molto in quel conduttore che avrà più lunghezza comunque sia d'altrettanto men grosso, cosicchè la quantità della superficie rimanga eguale: come WATSON ed altri aveano già osservato, e come io mi fusingo d'aver posto in miglior lume nella mia memoria sulla capacità de' conduttori semplici \*, nella quale dimostro il grande vantaggio di un conduttore costruito di molte verghe di legno coperte di foglia metallica, e collocate in lungo punta a punta,

\* Fu pubblicata questa memoria in un'opera periodica di Milano intitolata *Opuscoli Scelti*, per l'anno 1778, e nel Giornale dell' Ab. ROZIER l'anno seguente.

sopra gl' ordinarij conduttori affai più grossi e meno lunghi. Se l'istesso conduttore colla grossezza e lunghezza medesima non sia diritto, ma affai curvo, e molto più se essendo es. gr. un fit di ferro, abbia molti torcimenti, o si ripieghi indietro, avrà minore capacità; così pure l'avranno minore le indicate verghe, se invece d'esser collocate punta a punta in linea retta, lo siano ad angolo, e peggio se s'accostino parallele.

Le sperienze ed osservazioni da me rapportate in quello scritto, ed infinite altre, massimamente quelle intorno al così detto *pozzo elettrico*, concorrono tutte a provare, che la capacità è in ragione non delle superficie qualunque esse sieno, ma delle *superficie libere dall' azione delle atmosfere omologhe*: nella quale rettificata proposizione converranno tutti quelli, che si faranno a considerare i principali fenomeni delle atmosfere elettriche.

36. Ma v' è dippiù ancora: e questo è propriamente che fa al nostro caso. L'istesso conduttore ritenendo la stessa superficie, e la forma sua non mutata, acquista maggiore capacità allorchè in luogo di rimanere isolato solitariamente, si affaccia a un altro conduttore non isolato, e l'acquista tanto sempre maggiore quanto vi si affaccia più davvicino, e quanto le superficie che si presentano un l'altro sono più larghe. Io chiamo quel conduttore isolato che ne ha un altro di fronte (sia questo non isolato, come nel caso nostro, sia anche isolato, elettrizzato o no), lo chiamo *conduttore conjugato*; e già io aveva promesso nella mentovata dissertazione, trattato avendo della capacità de *conduttori semplici*, o *solitari*, di trattare in seguito di quella de *conduttori conjugati*.

37. Tale circostanza, che accresce prodigiosamente la naturale capacità di un conduttore, quella è sopra tutto, a cui non truovo che si sia fatta ancora la debita attenzione; molto meno che alcuno ne abbia tratto quei vantaggi, che dall' applicazione facilmente



facilmente ne derivano. Ma veniamo a quelle sperienze più semplici, che ci mettono sott' occhio questa accresciuta capacità.

Prendo un disco di metallo, il solito scudo d'elettroforo per esempio, e tenendolo in alto isolato lo elettrizzo a una data forza, quanto basta, supponiamo, a fare che un elettrometro annesso si tenda a 60 gradi; calando indi esso disco gradatamente verso un tavolo od altro piano deferente, ecco che decade l'elettrometro a 50, 40, 30 gradi. Non crediate perciò che sia scemata a questo punto la quantità d'elettricità che il disco possiede, la quale anzi, purchè quello non sia giunto a tale vicinanza dell' altro piano deferente da dar luogo alla trasfusione collo scoccare di qualche scintilla, si farà mantenuta nell' intiezza sua, quanto almeno la lunghezza del tempo, lo stato dell' aria e dell' isolamento lo permette. Onde dunque tale e tanto abbassamento di *tensione*? Non altronde che dall' accresciuta capacità del disco, or non più solitario, ma *conjugato*. In prova di che se si sollevi di nuovo gradatamente, risalirà il suo elettrometro a 40, 50, e fin presso ai 60 gradi di prima (risalirebbe a 60 giusto, se si potesse impedire affatto il dissipamento nell' aria, e lungo gl' isolatori non mai perfetti abbastanza); a misura cioè che allontanandosi dall' altro piano deferente ritorna il disco a quella più angusta capacità, che gli compete quand' è solitario.

38. La ragione di un tal fenomeno si deduce facilmente dall' azione delle *atmosfera elettriche*. Quella del disco, che or suppongo elettrico *per eccesso* si fa sentire al tavolo, od altro qualsivoglia conduttore, a cui si affaccia in guisa che il fuoco di questo, giusta le note leggi, ritirandosi si dirada nelle parti più vicine al disco sovrastante, e tanto più si dirada, quanto esso disco elettrico si va più accostando. Se l'elettricità di questo è *per difetto*, il fuoco del tavolo o piano inferiore qualunque sia,

accorre

accorre e si addensa verso la superficie medesima, che guarda il disco, e che ne sente più d'avvicino l'azione. Insomma le parti immerse nella sfera di attività del disco contraggono un elettricità *contraria*, elettricità che può dirsi *accidentale*, e che portando in certo modo un *compenso* a quella *reale* del disco medesimo, ne diminuisce la *tensione*, come appunto ci dimostra l'abbassamento dell' elettrometro (præc.)

39. Due altre sperienze porranno in maggior lume questa azione reciproca delle atmosfere elettriche, mercè di cui ora s'indeboliscono, ora si rinforzano mutuamente le *tensioni* ossia azioni elettriche di due corpi col solo avvicinarsi l'uno all' altro, ritenendo ciascuno nè più nè meno la sua dose di elettricità.

Cominciamo da quelle che si rinforzano. Queste sono le *atmosfere omologhe*. Siano pertanto due piani conduttori, due dischi, elettrizzati o per eccesso amendue, o amendue per difetto. Si affaccino questi, e si vadano gradatamente avvicinando: vedrassi che influiscono l'uno sull' altro in modo, che la *tensione* elettrica s'accresce in amendue a proporzione del più grande avvicinamento, e della quantità di superficie che si presentano: ciò, dico, vedrassi dal maggiore innalzamento de' rispettivi elettrometri, e dalla scintilla, che esplorando l'uno o l'altro di quei dischi scoccherà a maggior distanza, che se ciascuno fosse rimasto con tutta la sua elettricità *solitario*. In quello stato adunque di avvicinamento egli è chiaro, che ciascuno de' due conduttori *conjugati* ha una minore capacità; giacchè a proporzione che sono già *attuati* a un più altro grado di elettricità, lor resta meno per giugnere al sommo, o a parlar più giusto, maggiore è la resistenza che oppongono ad un ulteriore carica, conformemente a quanto osservato già abbiamo (33.) che la tensione esprime lo sforzo, onde un corpo tende a disfarsi dell' elettricità, e a comunicarla altrui. Così una boccia di

Leyden carica a un grado un poco maggiore di quello dei dischi solitari, la quale per conseguenza darebbe loro in tale stato, riceverà all' incontro da essi quando essendo *conjugati* vi prevale la *tensione*: ritornando questi solitari, cederanno un'altra volta alla boccetta, &c.

Or anche si comprende quello, che abbiamo fatto più sopra osservare (36.), onde sia cioè che un filo metallico ripiegato, o molte verghe poste allato e vicine le une alle altre, abbiano minore capacità che disposte l'une all' altre in una linea retta; perchè con superficie equali un conduttore corto e grosso abbia meno capacità d'un lungo e sottile; perchè infine la capacità sia in ragione delle superficie libere, o meno attuate dall' influsso delle atmosfere omologhe.

40. Siano ora i medesimi dischi della speriienza precedente ambi elettrizzati, ma uno *per eccesso* l'altro *per difetto*, ben si vede che ne seguiranno effetti contrarj: cioè l'influenza vicendevole delle atmosfere, per cui l'uno è *attuato* dall' altro, produrrà un *compenso* od *equilibrio accidentale*, onde diminuirassi la *tensione* in amendue, cadrà l'elettrometro, &c. Allora io dico che trovasi accresciuta in ciascuno de due dischi la capacità, inquantochè opporrà ciascuno minor resistenza ad un ulteriore carica dell' elettricità che già possiede, e gliene rimarrà dippiù a prendere per giugnere a un dato grado di *tensione*. Così una boccetta di Leyden carica dell' istessa specie d' elettricità d'uno di questi dischi, e all' istesso grado ed anche al disotto, potrebbe tuttavia aggiungere all' elettricità di quello, quando, trovandosi *conjugato*, la sua *tensione* è indebolita dall' atmosfera elettrica contraria del disco compagno; ma rimosso quello da questo, e divenuta in lui la *tensione prevalente*, darebbe egli della sua elettricità alla boccetta, &c.

41. Non resta più ora che fare un applicazione di quest' ultima speriienza a quelle riportate di sopra (38.), in cui il disco

elettrizzato si affaccia a un piano conduttore non isolato. S'egli è vero, come supposto abbiamo che questo nella parte più vicina a detto disco elettrico, per l'azione della di lui atmosfera, si compone ad un elettricità contraria, vale a dire che il fuoco ivi si dirada qualor l'incombente elettricità sia *in più*, o vi si condensa qualor sia *in meno* (39.), dovrà dunque nascere l'istesso *equilibrio* accidentale, l'istesso *compenso*, e alleviamento alla *tensione* elettrica del disco, lo stesso abbattimento dell' elettrometro, come appunto si osserva (38.): quindi l'accresciuta capacità di esso disco; quindi la maggior dose di elettricità che potrà ricevere (prec.) &c.

42. La cosa è già bastantemente chiara, ma si renderà ancora più manifesta, e toccherassi con mano, se si venga ad isolare il piano conduttore (supponiam che questo sia parimenti un disco metallico, che chiameremo disco inferiore) affacciato già al disco elettrico, e dopo si allontanino un dall' altro; giacchè allora compariranno realmente in esso piano o disco inferiore i segni dell' elettricità contraria da esso lui acquistata allorchè non era isolato, e trovavasi immerso nell' atmosfera del disco superiore. Cote sto disco superiore poi, il quale intanto ch'è si allontana, ricupera la *tensione*, che l'avvicinamento gli avea fatto perdere, la perderà di nuovo a misura che si accosterà un'altra volta al disco inferiore, e la farà perdere a lui medesimo, in virtù dell' azione reciproca delle contrarie elettricità (41.) a indicare le quali vicende è opportuno che trovisi un elettrometro annesso a ciascuno de' dischi; poichè il linguaggio dell' elettrometro è il più significante di tutti, e ardisco dire ch' esso solo vi dà la spiegazione di tutti i fenomeni riportati in questo scritto, e d'infiniti altri analoghi.

43. Che se il disco inferiore si truovi isolato, al primo affacciarvi il disco superiore elettrizzato, e isolato rimanga tutto il tempo che questo vi sta sopra, in tal caso venendo *attuato* dalla

di lui atmosfera, acquisterà quella che chiamo elettricità *omologa accidentale*, cioè una *tensione* od azione elettrica, con cui fa sforzo di conseguire l'elettricità contraria; il che non venendogli dato di effettuare, per l'isolamento in cui si truova, non potrà neppur *compensare* nel dovuto modo l'elettricità del disco incombente, nè quindi diminuire in lui la *tensione* notabilmente, dimodochè l'elettrometro appena farà cenno di abbassarsi (il qual picciolo abbassamento si deve a quel poco di fuoco, che per l'azione dell' atmosfera elettrica può muoversi nella spessezza del qualunque disco inferiore, o lungo i suoi sostegni isolanti non mai perfetti abbastanza); e per conseguenza non acquisterà il disco superiore maggiore capacità, onde poter prendere maggior dose di elettricità. Ma bene l'acquisterà, se un momento si venga a toccare il disco inferiore, onde distruggere in esso l'elettricità *accidentale omologa*, che vuol dire fargli prendere la *reale contraria*.

44. Se il disco inferiore non che trovarsi isolato, sia egli medesimo isolante, succederà lo stesso, cioè non potrà diminuire la *tensione* elettrica nè quindi aumentare la *capacità* del disco superiore accostatogli comunque. Non così però se cotal disco isolante farà semplicemente un sottile strato che copra un conduttore; mercecchè questo piano conduttore che trovasi poco sotto, e in cui può muoversi liberamente il fuoco, farà esso il giuoco di *compensare* l'elettricità del disco superiore; e lo strato isolante interposto diminuirà soltanto l'azion mutua delle atmosfere elettriche, in ragione della maggior distanza che pone tra l'uno e l'altro conduttore.

45. La *tensione* ossia azione elettrica del disco, la quale, come abiam veduto (38. 42.) va diminuendosi a misura ch' egli si affaccia più d'avvicino ad un piano deferente non isolato, è portata

tata a un tale decadimento quando si arriva quasi al contatto, il *compenso* od *equilibrio accidentale* essendo allora quasi perfetto, che dove l'elettrometro era teso a 60, 80, 100 gradi, si vedrà or disceso a 1 grado solo, ed anche meno. Quindi se il piano o disco inferiore opponga solo una picciola resistenza al trapasso dell' elettricità, o per l'interposizione d'un sottile strato coibente, o per la natura sua propria d'imperfetto conduttore, qual è il marmo asciutto, il legno secco, &c. tale picciola resistenza congiunta a quella della distanza comunque picciolissima non potrà essere superata da tale debolissima *tensione* del disco elettrico; il quale perciò non iscaglierà scintilla al piano (salvo che forse dagl' orli non ben ritondati, e nel caso che possieda una gran copia di elettricità); anzi conserverà tutta o quasi tutta la sua elettricità, dimodoche rialzandolo, il suo elettrometro ascenderà quasi al grado di prima. Più: potrà il disco senza gran detrimento della sua elettricità giugnere fino al contatto del piano imperfetto conduttore, e restarvi qualche tempo applicato: nel quale contatto la *tensione* elettrica trovandosi pressochè ridotta a nulla non ha forza di passare dal disco al piano che combacia se non lentissimamente.

46. Non andrà però così la bisogna, se ripetendo l'esperienza s'inclini il disco, e si porti a toccare il medesimo piano in costa: allora sussistendo in quello maggior *tensione* di elettricità (come ci mostrerà il fedele elettrometro), giacchè non vien bilanciata che corrispondentemente ai punti di superficie dell' uno che guardano davvicino la superficie dell' altro, cotal azione elettrica meno indebolita vincerà la piccola resistenza del marmo, o di qualsiasi altro imperfetto conduttore, e fino di un sottile strato coibente che trovisi interposto, cosicchè l'elettricità trasfonderassi realmente, e o s'affiggerà a cotesto strato coibente che copre il conduttore, o

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passerà

passerà entro a questo se è nudo fino a perdersi nel suolo \*, e ciò in brevissimo tempo: laddove vedemmo (præc.), che non ne passa nulla o quasi nulla in tempo affai più lungo, quando il contatto col medesimo piano è il più ampio possibile. Il che ha l'aria di paradosso; ma pur si spiega così bene coi principj delle atmosfere elettriche.

47. Quello che sembra anche più paradosso, o almeno che sorprende dippiù, si è che neppure il contatto di un dito, o di un pezzo di metallo comunicanti col suolo, replicato più volte e continuato per alcuni secondi, valga a spogliare intieramente

\* Questa spiegazione bene intesa ci conduce a render ragione in generale della *virtù della punta*. A parlar giusto una punta non isolata, presentata a un corpo elettrico non ha alcuna virtù propria per attrarne l'elettricità, ella si comporta semplicemente come un conduttore non isolato che non oppone resistenza al passaggio del fluido elettrico. Se il medesimo conduttore presenta al corpo elettrico in vece della punta una palla, od una superficie piana, non oppone già egli per questo maggiore resistenza; onde è dunque che l'elettricità non vi si getta egualmente all' istessa distanza dal corpo elettrico? Ciò viene dall' indebolita *tensione* ossia azione elettrica di cotesto corpo in virtù della più larga superficie presentatagli da quel conduttore non isolato, la quale superficie componendosi ad un elettricità contraria, offre maggior *compenso* che una punta, come si è qui sopra spiegato. Adunque in luogo di dimandare perchè una punta tragga o getti sì da lungi l'elettricità, dovrebbesi domandare piuttosto perchè una palla o un piatto egualmente conduttore non lo facciano: allora io farò osservare che non è già un difetto di questa palla o di questo piano, come non è una virtù propria della punta che metta tale e tanta differenza; ma bene lo stato del corpo elettrico e della sua atmosfera (con cui intendo anche l'aria che lo circonda *attuata* ad una tensione di elettricità omologa) il qual decade dalla sua forte *tensione* a proporzione che s'immergono in detta sua atmosfera e si affacciano a lui più punti di un conduttore non isolato. Affievolita pertanto l'*azione elettrica*, è egli sorprendente che non possa più superare la resistenza di quel lungo strato d'aria interposta tra il corpo elettrico ed il conduttore, che supera agevolmente quando non presentandogli alla medesima distanza che una punta sottile, la *tensione* di esso corpo elettrico e dall' aria infinitamente meno bilanciata, sussiste nel suo pieno vigore?

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dell' elettricità il disco posato full' amico piano ; ma ve ne lasci sovente tanto da poter dare ancora una scintilla quando in seguito si leva esso disco in alto. Invero tal fenomeno farebbe inesplicabile anche nei nostri principj, se il dito o il metallo fossero perfetti conduttori, a segno di non opporre la minima resistenza al passaggio del fluido elettrico, come si crede comunemente ; ma la cosa non è così ; e ce lo dimostrano queste stesse sperienze. I metalli dunque non sono che conduttori meno imperfetti degl' altri corpi. Ma, dirassi, noi vediamo che si trasfonde da un capo all' altro di un metallo, e da un metallo all' altro l'elettricità in un istante. Sia pure così di quell' elettricità che dispiega una forza sensibile a segno di tendere un elettrometro, o di attrarre un filo leggerissimo. Ma convien riflettere che al disotto di questo vi hanno da essere ancora altri gradi di elettricità impercettibili, i quali, dico io, non son vevoli a superare sì tosto quella qualunque piccola resistenza che pure oppor denno i migliori conduttori. Quando dunque un metallo tocca il disco elettrizzato che riposa sul suo piano, lo spoglia immantinente dell' elettricità fino al segno che la *tensione* diviene affatto *insensibile*, non però *nulla*, essendo ridotta supponiamo, a  $\frac{1}{16}$  di grado. Ma se sollevando il disco in alto la sua *capacità* si restringa a segno che dispieghi una tensione elettrica 100 e più volte maggiore, questa salirà dunque a 2 gradi, ed oltre ; con che sarà divenuta sensibile, finanche al punto di dare una scintilla.

48. Fin qui considerato abbiamo come l'azione delle atmosfere elettriche debba modificare l'elettricità del disco nelle sue varie situazioni, allorchè gli è stata infusa prima di accostarlo al piano deferente. Ora vediamo che avvenir debba allorchè gli s'infonde stando già egli vicino o meglio applicato al detto piano. Quando ho detto dal bel principio (32.) che in tale stato egli ha molto maggiore



maggior *capacità*, e son venuto provandolo fin qui, ho detto e provato tutto: le applicazioni sono facili a farsi. Gioverà non pertanto esemplificare un'esperienza. Mi si dia una boccia di Leyden, o un ampio conduttore elettrizzati a 1 sol grado di *tensione*, od anche meno. Se io farò toccare l'una o l'altro al mio disco posato, è chiaro che gli comunicheranno della loro elettricità a misura della sua *capacità*, tanto cioè quant'egli può riceverne per comporsi con essi ad una *tensione* ossia forza elettrica *eguale*, supponiamo di  $\frac{1}{2}$  grado. Ma la sua *capacità* or ch'egli è non solamente *conjugato* ma combaciante il conduttore compagno, è 100 e più volte maggiore (46.) di quando si trova isolato *solitariamente*, ossia vi vuole per produrvi la data tensione 100 volte maggior dose di elettricità (33.), quindi appunto ne avrà preso 100 volte più, che non avrebbe potuto prenderne stando isolato in aria. Quando dunque si leverà in alto a misura che allontanandosi dal caro piano si ridurrà alla naturale sua angusta *capacità*, la *tensione* elettrica dispiegherà più maggiore, e maggior sempre fino al termine di 50 gradi (nel supposto caso che la tensione fosse di  $\frac{1}{2}$  grado stando il disco posato), quando cioè la sua atmosfera non facendosi più sentire al detto piano, farà cessata ogni maniera di *compenso*, e tolto quell' *equilibrio accidentale*, che teneva la tensione così bassa (39. 42.). E inutile il dire, che calando di nuovo il disco verso il piano, si abatterà di nuovo l'elettrometro, a misura che l'*equilibrio accidentale* si andrà ristabilendo; giacchè questo è il primo fenomeno che contemplato abbiamo (38.), e che ne ha condotti alla spiegazione di tutto il resto.

49. Soggiugnerò questo per ultimo schiarimento. Succede al disco che passa dallo stato d'isolamento solitario a quello di affacciarsi fin anche a combaciare un piano convenientemente preparato, o da questo all'altro stato, lo stesso che succede ad un conduttore compreso

compreso sotto angusta superficie, che si dispieghi in una affai più ampia, e *vice versa* (richiamiamo l'esempio della catena ammucchiata e poi distesa, o dei cilindri ch' entrano un nell' altro (35.). Elettrizzato a un alto grado il conduttore quand' è avvolto e impicciolito, se dopo viene a distendersi od allungarsi, decade in lui la *tensione* a misura che l'elettricità, compartendosi a una più grande *capacità*, vien diradata. All' incontro elettrizzato debolmente quando è disteso e gode della sua maggiore capacità, se dopo si avvolge e rimpicciolisce, va egli acquistando viemmagior *tensione* a misura che l'elettricità si raccoglie e viene condensata in una capacità minore. Così appunto il nostro disco se venga elettrizzato quand' è *solitario* a una forte *tensione* questa andrà scemando a misura ch' egli si affaccia ad un altro piano non isolato; all' incontro elettrizzato debolissimamente quando è prossimo a questo piano o lo combacia, vedrassi crescere in lui insignemente la *tensione* a misura che si allontana da quel piano. Si può dunque dire che l'elettricità viene qui pure in certo modo *condensata*, non altrimenti che nell' addotto esempio del conduttore che s'impicciolisce: e quindi il nome di *condensatore* che ho dato al mio apparecchio. Certo se non può dirsi nel nostro caso *condensata* l'elettricità in minore spazio, giacchè e massa e volume rimangono i medesimi nel disco che adoperiamo, ella è però confinata in tal corpo di cui la *capacità* di grandissima che era è divenuta come che sia picciolissima.

50. Ora se una debole insensibile forza elettrica di una boccetta di Leyden o di un conduttore appena un poco carichi applicata al disco giacente può accumularvi tanto di elettricità, onde poi levato in alto dispieghi una forte tensione, vibri vivace scintilla, &c. che farà una carica forte della boccia o del conduttore applicatavi egualmente? Non farà gran cosa dippiù, per la ragione che tutta quell' elettricità ch' è superiore in forza  
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alla piccola resistenza che oppone la superficie del piano (46.), sia persa, trapassando in esso (47.). Ad ogni modo se questo piano essendo convenientemente preparato (11. 12. 22.), tale resistenza sia discreta, il disco non se ne staccherà senza vibrare d'attorno dagl' orli comunque ritondati fiocchi di luce, per la strabocchevole copia di elettricità, di cui si troverà carico: e a tanto non sarà neppur necessario che la boccetta che s'impiega a dargliela abbia affai forte carica, bastando una mediocre, e meno che mediocre, tale che appena giunga a dar scintilla.

51. Da tutto il fin qui detto s'intende facilmente, che se il disco posato può prendere buona dose di elettricità da una boccia di Leyden \*, o da un ampio conduttore, comechè debolissimamente animati, non lo può in alcun modo da un conduttore poco capace (e come darebbe questi cio che non ha?) a meno che non si continui d'altra parte ad infondere a lui medesimo

\* Nella mia memoria sulla capacità de' conduttori semplici dimostro la grandissima *capacità* che ha una boccia di Leyden comparativamente alla sua mole, appunto perchè l'elettricità che s'infonde ad una superficie trova un gran *compensò* nell' elettricità contraria che prende la superficie opposta, ciò che produce la solita diminuzione di *tensione*, &c. Vi fo vedere come 16 pollici quadrati di *superficie armata* hanno una capacità eguale a un conduttore di verghe inargentate lungo presso a 100 piedi, il quale ne ha una grandissima, talche le sue scintille producono la vera *commozione* in un grado abbastanza forte. Ivi anche accenno come tutti i fenomeni della carica e della scarica degli strati isolanti, dell' elettroforo, delle punte ec. possono dipendere dall' istessa azione delle atmosfere elettriche, combinata, per ciò che appartiene agli strati isolanti, con una certa non molto grande resistenza che prova l'elettricità ad affiggerli alla superficie di questi egualmente che a sortirne, e con quella incomparabilmente più grande e può dirsi insuperabile che la impedisce di diffondersi attraversandone la spessezza. Intorno a che fin dal tempo in cui pubblicai la descrizione, e le principali sperienze del mio elettroforo, che fu nel 1775 (vegg. la Scelta d' Opusc. interes. di quell' anno) io avea promesso di esporre tutte le mie idee in un trattato che avrebbe per titolo: *dell' azione delle atmosfere elettriche, e de' fenomeni che ne derivano negli strati isolanti.*

quella

quella qualunque debole elettricità, a meno che la sorgente non continui per qualche tempo: il che ha luogo per esempio nel conduttore atmosferico che bee l'elettricità insensibile dell'aria, e in quello malissimo isolato d'una macchina ordinaria, il di cui giuoco vi mantiene una sì debole *tensione* di elettricità, che in niun modo appara. In ambi questi casi abbiamo osservato infatti (4. 25.) che vi vuol del tempo prima che il disco possa raccogliere una dose sufficiente di elettricità.

52. Come un ampio conduttore trasmette la massima parte della sua elettricità al nostro disco, il quale quantunque assai più picciolo, gode però in grazia della sua vantaggiosa posizione, in grazia di quell' *equilibrio accidentale* a cui si compone col piano, d'una *capacità* molto più grande di quella che gli compete in istato solitario; e come levando in seguito esso disco in alto, con che tolto ogni *equilibrio* o *compenso*, vien ristretto alla naturale sua angusta capacità, quella stessa dose di elettricità presa al gran conduttore, e che appunto per esser egli sì grande vi producea sì debole *tensione*, or ne produce una tanta più grande in cotesto disco; nell' istessa maniera, e per l'egual ragione l'elettricità aumenterà una seconda volta di tensione facendola passare dal disco già sollevato ad un altro giacente molto più picciolo, da innalzarsi quindi similmente.

Il Sig. CAVALLO, a cui dietro le altre mie sperienze, suggerì quest'artificio, ha fatto tal picciolo disco d'una lamina non più grande d'uno scellino. E certo questo secondo *condensatore* dell' elettricità è utile in molti casi in cui l'elettricità non è sensibile ancora o dubbia col primo: come ce ne hanno assicurato varie prove che facemmo insieme. Talora l'ordinario disco toccato dal corpo, di cui si dubitava se avesse o no un principio di elettricità, non movea ancora l'elettrometro, sensibilissimo dell' istesso Sig. CAVALLO; ma toccato con quel disco l'altro picciolino, questo faceva divergere sensibilmente le

palottoline dell' elettrometro. Eppure qualche volta anche con questo non si otteneva nulla, o un' ombra solamente di elettricità. Or se noi supponiamo la *tensione* elettrica accresciuta a 1000 volte tanto per l'intervento dei due condensatori, il che non è troppo, quanto mai debole esser dovea originariamente nel corpo esaminato? Quanto debole p. e. quella che si eccita in un metallo strofinandolo colla mano nuda, giacchè comunicata al primo, e da questo al secondo picciolo disco, e finalmente all' elettrometro, le palle appena fan cenno di scostarsi? Ma basta che facciano tanto per esser noi convinti, che l'elettricità non è nulla, e che il metallo l'ha originariamente contratta per lo stroppicciamento della mano. Quanto mai eravam lontani da una simile scoperta pochi anni addietro prima del nostro *condensatore*, e dell' elettrometro così sensibile del Sig. CAVALLO. Quanti gradi di elettricità noi scopriamo adesso al disotto del più picciolo d'allora?

## A P P E N D I C E.

HO detto al § 28. che mi è riuscito finalmente di ottenere segni distintissimi di elettricità e dalla semplice evaporazione dell' acqua, e da varie effervescenze chimiche. Essendo questo un fatto non meno interessante che nuovo, stimo non inopportuno di far qui il racconto fedele delle sperienze. Le prime dunque, come ivi accenno, sono state fatte a Parigi in compagnia di due fisici illuminati e membri dell' Acc. R. delle Scienze. Furono questi il Sig. LAVOISIER, e il Sig. DE LA PLACE. Eglino concepiron meco la speranza di un felice riuscimento  
quando

quando ebbi loro mostrato gli effetti del mio *condensatore*, e spiegata la ragione dei fenomeni: conseguentemente il Sig. LAVOISIER ne ordinò un grande col piano di marmo bianco. I primi tentativi da me fatti con questo in compagnia del Sig. DE LA PLACE full' evaporazione dell' acqua e dell' etere non furono coronati dal successo; ma il tempo era cattivo, la stanza troppo picciola e ingombrata di vapori, e l'apparato non troppo ben in ordine. All' incontro quelli che ripeterono l'istesso Sig. DE LA PLACE e Sig. LAVOISIER ad una campagna di quest' ultimo ebbero buon riuscimento. La qual cosa c'invogliò a ripetere e moltiplicar le sperienze, e il successo fu completo, avendo ottenuto segni chiarissimi di elettricità dall' evaporazione dell' acqua, dalla semplice combustione dei carboni, e dall' effervescenza delle limature di ferro nell' acido vitriolico diluto. Ciò avvenne il giorno 13 Aprile e la maniera di far l'esperienza fu questa: si isolò in un aperto giardino una gran lastra di metallo, alla quale era attaccato un lungo filo di ferro che veniva a terminare in contatto dello scudo o disco posato sul piano di marmo, e questo tenevasi continuamente asciutto e caldo da alquanti carboni sottoposti. Ciò fatto posimo su la detta lastra isolata alcuni scaldini ripieni di carboni mezzo accesi, e lasciammo che la combustione ajutata da un gentil vento che spirava andasse rinforzandosi per alcuni minuti: allora rimuovendo lo scudo dal contatto del filo metallico e quindi da quello dal marmo con alzarlo al consueto modo, vi comparvero i segni aspettati di elettricità, mentre accostato al nuovo elettrometro del Sig. CAVALLLO, fece che s'aprissero i due fili colle pallottoline: esaminata questa elettricità si trovò essere *negativa*. Si ripeté l'esperienza ponendo sulla lastra isolata invece dei scaldini quattro vasi con entro limatura di ferro e acqua, quindi versando in tutti quattro a un tempo abbastanza d'olio di vitriolo per far forgere.

una furiosa effervescenza: quando il più forte bollorè cominciava a cadere, allora fu che esplorato lo scudo non che muovere i fili dell' elettrometro a qualche distanza, ci diede una sensibile scintilla. Anche qui l'elettricità si riconobbe essere *negativa*. Quanto furon vivi e distinti i segni elettrici con tal prova dell' effervescenza, altrettanto deboli ed equivoci riuscirono questa volta coll' evaporazione dell' acqua eccitata or con mettere delle casserole con entro acqua a bollire sopra i scaldini portati come qui innanzi dalla lastra isolata, ora con versar l'acqua in coteste casserole previamente ben riscaldate.

Pochi giorni dopo ripetemmo le sperienze in una grande stanza estendendole alle altre effervescenze che producono l'aria fissa, e l'aria nitrosa, con buon successo: l'evaporazione sola dell' acqua produsse segni debolissimi talchè ebbimo pena a determinare di quale specie fosse l'elettricità; anzi di tre volte, due ci parve che fosse *positiva*; ma v' è luogo a credere, ed io giudico certamente, che sia stato un errore.

Ancor passati alcuni giorni si ritornò alle sperienze essendo di compagnia anche il Sig. LE ROY membro esso pure dell' Accademia R.; ma né la combustione, né l'evaporazione dell' acqua non ci dieder segni sensibili: di che accagionammo l'esser l'aria umidissima per il tempo piovoso che faceva. Pur ne ottenemmo colla generazione dell' aria infiammabile nel momento della più viva effervescenza: e se l'elettricità non fu questa volta così forte da scintillare, lo fu abbastanza perchè ne distinguessimo chiarissimamente la specie, che era *negativa*.

Prima di lasciar Parigi (che fu il 23 Aprile) volendo io mostrare qualche sperienza di questo genere ad un amatore di elettricità e valente machinista, il Sig. BILLAUM, una volta che mi trovai nel suo laboratorio, presi una giara di vetro, e sospesala a un cordoncino di seta vi misi i materiali per la produ-

zione dell' aria infiammabile: avea fatto entrare nella giara medesima un filo di ferro in modo che toccasse la limatura e l' altro suo capo sporgente venisse a comunicare coll' elettrometro sensibilissimo del Sig. CAVALLO. Quando l'effervescenza fu salita al sommo e la spuma formontava i labbri del vaso, le palle, scostandosi, dieder segno di elettricità; nè questa fu così debole, che non potesse conoscersi esser *negativa*.

Le sperienze coll' evaporazione dell' acqua, che non avean troppo bene corrisposto a Parigi, ebbero molto miglior successo a Londra, quando mi suggerì l'espedito di gettere dell' acqua sopra i carboni accesi ch' erano in uno scaldino isolato. L'effumazione rapida che succede non manca mai di elettrizzare lo scaldino *negativamente*, il quale dà segni abbastanza sensibili col solo elettrometro, e col condensatore, se è ben preparato, arriva a produr scintille. Si trovarono presenti la prima volta a queste sperienze in casa del Sig. BENNET grand' amatore di elettricità, il Sig. CAVALLO e il Sig. KIRWAN membri della S. R. e il Sig. WALKER lettore di fisica. Ci servimmo per apparecchio condensatore d'un picciolo scudo d'elettroforo, è d'un piattello di legno, che si trovò al giusto punto semicoibente, il che è raro quando il leguo non è inverniciato.

Un' altra volta in casa del Sig. CAVALLO riuscì l'esperienza isolando un picciolo crogiuolo con entro due o tre carboni accesi e quindi versandovi un cucchiaino d'acqua: un filo di ferro che toccava i carboni, ed estendevasi fino all' elettrometro, vi portò sensibile elettricità e sempre *negativa*.

Queste sono le sperienze, che fino ad ora ho avuto occasione di fare; intorno alle quali non debbo tralasciar di dire, che sebbene non avessimo sempre bisogno dell' apparecchio *condensatore* (il quale, se non è benissimo in ordine, a nulla serve, e può nuocere anzichè giovare) per aver segni non dubbi, il solo

elet-



elettrometro sensibilissimo del Sig. CAVALLLO avendoci bastato più volte; convien però confessare che si fu quell' apparecchio che ci mise sulla via di tali sperienze, e che col mezzo suo solamente potemmo ottenere segni di una certa forza, e fin la scintilla elettrica. Io non dubito che essendo ora rese così facili tali sperienze, non siano per essere e ripetute e promosse. Il campo è solamente aperto, e molto resta ancora a fare. Se i corpi risolvendosi in vapori o in un fluido elastico si caricano di fuoco elettrico a spese degli' altri corpi, e gli elettrizzano per conseguenza *negativamente*, venendo in seguito i vapori medesimi a condensarsi, non cercheranno essi di deporre questo carico, e non produrranno conseguentemente segni di elettricità *positiva*? Ecco ciò che merita singolarmente d'essere verificato coll' esperienza. Io ho già immaginato diversi modi di tentare la cosa che metterò alla prova tosto che ne abbia il comodo. Intanto mi sia qui permesso di dar corso per un momento alle idee che volgo in mente intorno all' elettricità atmosferica.

Le sperienze fatte fin qui, e che abbiamo riferite, benchè non sian molte, tutte però concorrono a mostrarci che i vapori dell' acqua, e generalmente le parti d'ogni corpo, che si staccano volatilizzandosi, portano via seco una quantità di fluido elettrico a spese dei corpi fissi che rimangono, elettrizzandoli con ciò *negativamente*, non altrimenti che ne portan via una quantità di fuoco elementare, con ciò raffreddandoli. Quindi volli inferire che i corpi risolvendosi in vapori, o prendendo l'abito aereo, acquistino una maggiore capacità rispetto al fluido elettrico, giusto come l'acquistano maggiore rispetto al fuoco comune o fluido calorifico. Chi non sarà colpito da così bella analogia, per cui l'elettricità porta del lume alla novella dottrina del calore, e ne riceve a vicenda? Parlo della dottrina dal calor *latente* o *specifico*, come si vuol chiamare, di cui BLACK e

WILKE

WILKE colle stupende loro scoperte han gettato i semi, e che è stata ultimamente tanto promossa dal Dr. CRAWFORD dietro le sperienze del Dr. IRWINE.

Seguendo questa analogia siccome i vapori allorchè si condensano e ritornano in acqua, e conseguentemente alla primiera più angusta capacità, perdono il lor calore *latente*, ossia depongono il dippiù di fuoco che si avevano appropriato volatizzandosi; così pure darau fuori il fluido elettrico divenuto ora ridondante. Ed ecco come nasce l'elettricità di eccesso, che domina sempre più o meno nell' aria anche serena, a quell' altezza che i vapori cominciano a condensarsi; la quale è più sensibile nelle nebbie, ove quelli si condensano maggiormente, e già si figurano in goccie; e infino fortissima laddove le folte nebbie si agglomerano in nubi. Fin quì l'elettricità dell' atmosfera sarà sempre *positiva*. Ma formata che sia una nube potentemente elettrica in più, ella avrà una sfera di attività intorno ad essa, nella quale se avviene ch' entri un' altra nube, allora giusta le note leggi delle atmosfere, gran parte del fluido elettrico di questa seconda nube si ritirerà verso l'estremità più lontana dalla prima, e potrà anche sortirne ove incontri o altra nube, o vapori, o prominenze terrestri che lo possan ricevere: ed ecco una nube elettrizzata *negativamente*, la quale potrà a sua posta occasionare coll' influsso della propria atmosfera l'elettricità *positiva* in una terza, &c. di questa maniera s'intende benissimo come si possano avere sovente nè conduttori atmosferici segni di elettricità *negativa* a celo più che coperto; e come ne' temporali specialmente, ove molte nubi si veggono pensili e staccate vergere al basso, e or ondeggiare per qualche tempo, ora scorrere le une sotto le altre, or trasportarsi rapidamente, l'elettricità cambj più volte, e spesso a un tratto da *positiva* in *negativa*, e vice-versa.

Or

Or anche non fia più stupore che le eruzioni de vulcani, siano state sovente accompagnate da fulmini in ispecie. Quella strepitosissima del Vesuvio dell' anno 1779, in cui infinite faette si son vedute guizzare entro gl' immensi globi di fumo eruttati. Le poche sperienze fatte mi han dato a vedere che la quantità di elettricità prodotta dalle effumazioni, dipenda molto e dalla copia dei fumi che s'alzano e singolarmente dalla rapidità. Or quale e quanta non dee essere l'elettricità in simili eruzioni?

XVII. *Extract of a Register of the Barometer, Thermometer,  
and Rain, at Lyndon, in Rutland, 1780. By Thomas  
Barker, Esquire.*

Read April 11, 1782.

The ground was unusually dry the first half of January, and want of water in some places. Inclined to frost, but not fixed; one for a week was followed toward the end of the month by a smarter but shorter with a good deal of snow, and went away with rain and floods; then showery, and in February often stormy. The month of March was drier, generally fine, a good seed-time, and the middle of the month warm and growing; but cold N.E. winds at the end. Soon after April came in, the weather was fine and growing, sometimes showery, and the middle being very warm brought on things very much; but the end of the month, and beginning of May, cold N.E. winds, yet sometimes hot sun. About the middle of May was showery and growing; but the end of it, and beginning of June, hot, dry, clear, and burning. June 3d, began showers and often thunder, which being several times repeated for a fortnight, and hot weather with it, made plenty of grass in this country and some others; but where the rains were smaller, as they were about London, in Suffex, and other places, it only freshened the ground without growing much. The beginning of hay-time was very fine, a wet week in the middle did not greatly hurt the hay, and was very good for the turnips, then newly sown. The latter half of July, and almost all August, were very dry, hot, and burning; the harvest almost all very well got, except some of the pease, which were either not got in, or the hovels not thatched, when the great rain came September 2. There was a great crop of wheat, but in some places it was mildewed; the barley was universally good; the pease uncertain. The general prices were, wheat thirty-eight shillings, barley sixteen or seventeen shillings, oats eleven or twelve shillings, pease and beans about twenty-two or twenty-three shillings a quarter.

The ground was now exceedingly burnt, and great want of water; but in the first half of September there came so much rain, the ground already warm, and hot weather with it, that in a fortnight's time there was plenty of grass. The autumn was in general fine and pleasant, and the latter end of September and all October scarce any rain, so that at the end of the month the grass in some places plainly began to burn, which was remarkable so late in the year, and after its being so fruitful in September; and notwithstanding some showery weather in the former part of November, the roads were rather dusty the beginning of December; then dark, misty, raw, cold east winds, but milder again and showery till the end of the year. It has been an uncommonly open season, there have been some frosty mornings, and one day or two it scarce went away in the shade; but there has not been one thorough frosty day yet, and there has been thunder at Christmas.



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## METEOROLOGICAL JOURNAL

for January 1781.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	45,0	47,0	29,80		SW	1	Cloudy.
	2	0	46,0	47,0	29,71		SW	1	Cloudy and rain.
2	8	0	34,5	43,5	29,57	0,045	W by N	1	Fair.
	2	0	40,0	45,0	29,61		NW	1	Fine.
3	8	0	33,5	38,0	29,76		N by W	1	Fair.
	2	0	38,0	39,0	29,88		N by W	1	Fine.
4	8	0	28,0	35,0	30,01		NW	1	Fair and frosty.
	2	0	34,0	36,0	30,05		NW	1	Cloudy.
5	8	0	25,0	32,0	30,20		SSW	1	Frofty.
	2	0	32,0	33,0	30,23		SW	1	Frofty.
6	8	0	35,0	33,5	30,21		SSW	1	Cloudy.
	2	0	40,0	36,5	30,25		SW	1	Cloudy.
7	8	0	38,5	37,0	30,22		SSW	1	Cloudy.
	2	0	46,0	40,5	30,10		W by S	1	Fair.
8	8	0	40,5	41,5	30,31	0,042	N by E	1	Fair.
	2	0	42,5	41,5	30,39		ENE	2	Cloudy.
9	8	0	32,5	37,5	30,54		NNE	2	Fair.
	2	0	37,5	38,5	30,55		NE	2	Fine.
10	8	0	32,0	34,0	30,52		E by N	2	Cloudy.
	2	0	33,5	35,0	30,50		E by N	2	Cloudy.
11	8	0	32,0	33,5	30,38		E by N	3	Cloudy.
	2	0	31,0	33,5	30,32		NE	3	Cloudy.
12	8	0	31,5	32,0	30,26		ESE	1	Foggy.
	2	0	35,0	35,0	30,24		SE	1	Cloudy.
13	8	0	33,0	34,0	30,11		ENE	1	Foggy.
	2	0	35,0	35,0	30,16		NE	1	Cloudy.
14	8	0	29,0	32,0	30,04		E by N	1	Foggy.
	2	0	34,0	35,0	29,98		ENE	1	Cloudy.
15	8	0	30,5	33,5	29,92		ENE	1	Foggy.
	2	0	36,0	35,0	29,89		NE	1	Fine.
16	8	0	31,0	33,5	29,89		ENE	1	Foggy.
	2	0	33,5	33,5	29,83		NE	1	Cloudy.

M E T E -

## METEOROLOGICAL JOURNAL

for January 1781.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	32,5.	33,0	29,82		ENE	1	Foggy.
	2	0	38,0	35,0	29,76		NE	1	Rainy.
18	8	0	40,0	37,0	29,57	0,190	E by N	1	Rainy.
	2	0	45,0	39,0	29,56		NE	1	Cloudy.
19	8	0	37,5	40,0	29,78	0,221	NNE	1	Fine.
	2	0	41,0	41,5	29,86		NNE	1	Fine.
20	8	0	34,0	38,0	30,05		SE	1	Foggy.
	2	0	41,0	40,0	29,99		SE	1	Fine.
21	8	0	44,0	40,0	29,45		SW	1	Foggy.
	2	0	41,5	42,5	29,44		NNE	1	Rainy.
22	8	0	32,0	38,0	29,65	0,073	SE	1	Fair.
	2	0	33,0	38,0	29,66		SE	1	Fine.
23	8	0	26,0	32,0	29,39		ENE	1	Foggy.
	2	0	32,0	34,0	29,37		SE	1	Cloudy with snow.
24	8	0	35,5	34,0	29,16		SW	1	Rainy.
	2	0	41,0	37,0	29,04		SW	1	Rainy.
25	8	0	31,5	33,0	29,32		NW	1	Fair.
	2	0	35,0	36,0	29,52		NW	1	Fine.
26	8	0	35,0	34,0	29,09	0,485	SSE	1	Rainy.
	2	0	37,0	38,0	29,23		SE	1	Cloudy.
27	8	0	26,5	32,5	30,05		WSW	1	Fine.
	2	0	36,0	37,0	30,13		SW	1	Fine.
28	8	0	46,5	37,0	29,93	0,063	SW	2	Cloudy.
	2	0	51,5	42,0	29,94		SSW	2	Cloudy.
29	8	0	46,5	43,0	29,89		SSW	2	Cloudy.
	2	0	50,5	46,0	29,89		SW	2	Cloudy.
30	8	0	47,0	47,5	29,75	0,229	SW	2	Cloudy.
	2	0	48,0	49,0	29,81		SW	3	Showery.
31	8	0	35,5	43,5	30,22		WNW	1	Fine.
	2	0	45,0	44,0	30,22		SW	1	Fine.

M E T E-

## METEOROLOGICAL JOURNAL

for February 1781.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inche.	Inch.	Points.	Str.	
Feb. 1	8 0	42,0	43 0	30,05	0,110	SSW	1	Rainy.
	2 0	49,0	46,0	30,01		NW	1	Fine.
2	8 0	40,5	43,0	30 19	0,120	SW	1	Foggy.
	2 0	46,0	46,0	30,12		NW	1	Rainy.
3	8 0	41,5	44,0	30,33		SSW	1	Cloudy.
	2 0	43,0	46,0	30,34		SW	1	Cloudy.
4	8 0	43,0	46,0	30,34		SW	1	Cloudy.
	2 0	48,0	47,0	30,05		SW	1	Fine.
5	8 0	44,5	44,0	29,78		SW	1	Fine.
	2 0	50,0	49,0	29,75		SW	1	Cloudy.
6	8 0	46,5	49,0	29 88		SW	1	Cloudy.
	2 0	52,0	51,0	29,96		SW	1	Fine.
7	8 0	48,5	46,5	30,03		WSW	1	Fine.
	2 0	40,0	43,0	29,73		SW	2	Cloudy.
8	8 0	47,5	48,5	29,51	0,233	SSE	2	Rainy.
	2 0	52,0	50,5	29,59		SW	2	Fine.
9	8 0	40,0	46,0	29,82		SW	1	Fine.
	2 0	52,5	50,0	29 83		SW	1	Cloudy.
10	8 0	48,0	49,0	29,88		SSW	3	Cloudy.
	2 0	50,0	50,0	29,74		SW	3	Cloudy.
11	8 0	42,5	47,0	29,58	0,081	SW	2	Fine.
	2 0	51,0	50,5	29 59		SW	2	Fine.
12	8 0	47,0	49,0	28,95	0,430	SW	3	Cloudy.
	2 0	44,0	46,0	29,96		SW	1	Cloudy.
13	8 0	43,0	45 0	29,36		SW	2	Cloudy.
	2 0	47,0	47,5	29,39		NW	2	Fine.
14	8 0	39,5	45,0	29 59	0,150	WSW	1	Cloudy.
	2 0	44,5	44,0	29,52		NW	2	Fine.
15	8 0	35,0	41,0	29,98		NW	1	Fine.
	2 0	43 5	44,0	30,05		SW	1	Fine.
16	8 0	33,0	37,0	30,09		WSW	1	Fine.
	2 0	44,0	42,0	30,02		NW	1	Fine.

## METEOROLOGICAL JOURNAL

for February 1781.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	31,5	37,5	30,12		NW	1	Fine.
	2	0	42,0	44,0	30,07		NW	2	Fine.
18	8	0	31,0	36,0	30,09		N by W	1	Fair.
	2	0	43,5	40,5	30,03		SW	1	Cloudy.
19	8	0	37,0	39,0	29,79		NW	1	Fine.
	2	0	43,5	42,5	29,81		NW	1	Cloudy.
20	8	0	37,0	39,0	30,22		SW	1	Cloudy.
	2	0	42,5	41,5	30,23		NE	1	Fine.
21	8	0	34,0	38,0	30,16		NNE	1	Cloudy.
	2	0	35,5	39,5	30,09		NE	1	Rainy.
22	8	0	33,0	36,0	30,12	0,073	NNE	1	Fine.
	2	0	41,5	39,0	30,14		NNE	1	Fine.
23	8	0	37,5	37,0	30,03		NW	1	Cloudy.
	2	0	43,5	41,0	29,89		NW	1	Cloudy.
24	8	0	40,5	42,0	29,71	0,131	SW	1	Cloudy.
	2	0	49,0	44,0	29,71		SW	1	Rainy.
25	8	0	43,0	45,0	29,17	0,150	NW	1	Fine.
	2	0	44,0	46,5	29,19		NW	1	Fine.
26	8	0	37,0	41,0	29,08		SW	1	Cloudy.
	2	0	41,5	43,0	29,08		SW	1	Fine.
27	8	0	39,0	41,0	29,26	0,072	SW	1	Cloudy.
	2	0	45,0	47,0	29,88		NW	3	Cloudy.
28	8	0	37,5	41,0	29,79	0,126	NW	1	Fine.
	2	0	45,0	46,0	29,97		NW	1	Fine.

## METEOROLOGICAL JOURNAL

for March 1781.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			inches.	Inch.	Points.	Str.	
Mar.	1	7 0	35,0	41,5	30,19	SW	1	Fine.
		2 0	47,0	43,5	30,06	SW	1	Rainy.
	2	7 0	41,0	43,0	30,26	WSW	1	Cloudy.
		2 0	50,5	47,0	30,26	NW	1	Fine.
	3	7 0	48,0	49,0	30,26	SW	1	Cloudy.
		2 0	55,0	51,0	30,26	WSW	1	Cloudy.
	4	7 0	47,0	51,0	30,18	SW	1	Cloudy.
		2 0	50,5	51,5	30,10	SW	1	Cloudy.
	5	7 0	45,5	51,5	30,14	SW	1	Cloudy.
		2 0	51,0	51,5	30,22	NE	1	Cloudy.
	6	7 0	37,0	47,0	30,36	SW	1	Fine.
		2 0	52,5	51,0	30,33	SW	1	Fine.
	7	7 0	35,5	45,5	30,25	SW	1	Cloudy.
		2 0	51,0	49,0	30,19	WSW	1	Fine.
	8	7 0	37,5	40,0	30,11	SSW	1	Rainy.
		2 0	48,0	47,5	30,19	SSW	1	Cloudy.
	9	7 0	43,0	48,5	30,22	SW	1	Fine.
		2 0	55,0	50,5	30,17	SSW	1	Fine.
	10	7 0	44,5	49,5	30,17	WSW	1	Fine.
		2 0	55,0	52,0	30,17	NW	1	Cloudy.
	11	7 0	38,5	48,5	30,19	NNE	1	Fine.
		2 0	51,5	50,0	30,21	NNE	1	Fine.
	12	7 0	32,0	42,0	30,27	SE	1	Cloudy.
		2 0	42,0	46,0	30,28	NW	1	Cloudy.
	13	7 0	41,0	44,5	30,32	NE	1	Fine.
		2 0	49,5	48,0	30,31	SE	1	Fine.
	14	7 0	32,5	38,5	30,33	NE	1	Cloudy.
		2 0	45,5	44,0	30,33	ESE	1	Fine.
	15	7 0	33,5	37,0	30,44	NE	1	Fine.
		2 0	48,5	47,0	30,46	ENE	1	Fine.
	16	7 0	36,0	37,5	30,39	ENE	1	Cloudy.
		2 0	46,5	46,0	30,34	ENE	1	Fine.

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## METEOROLOGICAL JOURNAL

for March 1781.

	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 17	7	0	39,0	38,5	30,22		ESE	1	Cloudy.
	2	0	50,5	48,0	30,16		ENE	1	Fine.
18	7	0	33,0	41,5	30,09		SSE	1	Foggy.
	2	0	57,0	47,0	30,06		WSW	1	Fine.
19	7	0	39,0	46,5	30,14		NW	1	Fine.
	2	0	55,0	49,0	30,14		WSW	1	Fine.
20	7	0	41,5	44,5	30,22		NNW	1	Foggy.
	2	0	60,0	51,0	30,22		NW	1	Fine.
21	7	0	45,0	50,0	30,18		W by S	1	Fine.
	2	0	57,5	53,0	30,16		WSW	1	Fine.
22	7	0	41,5	46,5	30,19		W SW	1	Fine.
	2	0	58,0	53,5	30,18		SW	1	Fine.
23	7	0	41,5	46,5	30,38		SE	1	Fine.
	2	0	51,0	50,5	30,35		ENE	1	Fine.
24	7	0	36,0	42,0	30,47		NW	1	Foggy.
	2	0	56,0	49,0	30,47		SE	1	Fine.
25	7	0	38,0	46,0	30,46		NE	1	Fine.
	2	0	60,5	51,0	30,29		SW	1	Fine.
26	7	0	39,0	43,0	29,91		WSW	1	Fine.
	2	0	47,0	50,5	30,01		NE	2	Rainy.
27	7	0	48,5	43,5	30,02		NW	1	Fine.
	2	0	49,0	44,0	29,93		NW	2	Fine.
28	7	0	34,0	39,0	29,88		NE	1	Fine.
	2	0	48,0	45,0	29,89		NE	2	Fine.
29	7	0	34,0	37,0	29,92		NE	2	Cloudy.
	2	0	43,0	42,0	29,91		NE	2	Fine.
30	7	0	35,5	37,5	29,91		NE	2	Fine.
	2	0	47,0	42,0	29,91		NE	2	Fine.
31	7	0	35,5	35,0	30,09		ENE	1	Foggy.
	2	0	34,0	43,5	30,09		NE	2	Cloudy.

## METEOROLOGICAL JOURNAL

for April 1781.

	Time		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 1	7	0	37,5	38,0	29,91		ESE	1	Fine.
	2	0	37,0	43,0	29,81		E by S	1	Fine.
2	7	0	37,0	42,0	29,74		SW	1	Fine.
	2	0	53,5	47,0	29,83		ENE	1	Fine.
3	7	0	37,5	44,5	29,98		NE	1	Fine.
	2	0	53,5	52,0	29,94		SE	1	Fine.
4	7	0	38,0	43,0	29,76		NE	1	Cloudy.
	2	0	43,5	44,0	29,63		NE	1	Cloudy.
5	7	0	40,0	43,5	29,34		SE	1	Cloudy.
	2	0	51,0	48,0	29,29		SE	1	Cloudy.
6	7	0	45,0	48,5	29,37		SW	1	Cloudy.
	2	0	50,5	49,5	29,38		SW	1	Fine.
7	7	0	46,0	49,0	29,57		SW	1	Fine.
	2	0	51,0	51,5	29,51		SE	1	Rainy.
8	7	0	48,0	50,5	29,53	0,150	SW	2	Cloudy.
	2	0	50,0	65,0	29,68		SW	2	Fine.
9	7	0	53,0	54,0	29,72	0,200	SW	2	Rainy.
	2	0	50,0	60,0	29,82		NW	2	Fine.
10	7	0	54,0	57,5	29,74		SW	1	Rainy.
	2	0	62,5	60,0	29,76		SW	2	Fine.
11	7	0	49,0	51,5	29,73	0,140	SE	1	Fine.
	2	0	57,0	56,0	29,66		SE	1	Cloudy.
12	7	0	49,0	54,5	29,52		NW	1	Cloudy.
	2	0	61,5	58,0	29,64		NW	2	Cloudy.
13	7	0	39,5	51,0	29,96	0,095	SSW	1	Fine.
	2	0	54,0	53,5	29,99		SW	1	Fine.
14	7	0	40,0	45,0	29,91		WSW	1	Fine.
	2	0	58,0	52,5	29,79		SW	1	Fine.
15	7	0	46,0	47,0	29,79		SW	1	Fine.
	2	0	62,0	66,5	29,81		SW	1	Fine.
16	7	0	46,0	49,0	29,88		SW	1	Fine.
	2	0	53,0	58,5	29,86		SSW	1	Fine.

## METEOROLOGICAL JOURNAL

for April 1781.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	49,0	53,5	29,91		SW	1	Fine.
	2	0	66,5	61,0	29,92		SSE	1	Fine.
18	7	0	48,0	53,0	29,94		ENE	1	Fine.
	2	0	65,0	62,5	29,91		ESE	1	Fine.
19	7	0	55,5	57,5	29,93		SSW	1	Fine.
	2	0	65,0	62,5	29,99		WSW	1	Fine.
20	7	0	51,0	67,0	30,02	0,065	SSE	1	Cloudy.
	2	0	64,0	64,0	30,09		W	1	Fine.
21	7	0	55,0	58,0	30,18		SSW	1	Rainy.
	2	0	64,0	63,0	30,16		SW	1	Cloudy.
22	7	0	55,5	58,0	30,09		SSW	1	Fine.
	2	0	66,0	64,0	30,05		SW	1	Fine.
23	7	0	48,0	52,0	30,07		SW	1	Fine.
	2	0	57,0	57,0	30,01		NW	1	Cloudy.
24	7	0	45,0	47,0	30,16		NNE	1	Cloudy.
	2	0	51,0	55,0	30,22		NE	2	Cloudy.
25	7	0	44,5	50,0	30,24		NW	1	Fine.
	2	0	46,5	49,0	30,34		NW	1	Fine.
26	7	0	43,0	47,0	30,32		NW	1	Fine.
	2	0	53,0	53,0	30,32		NW	2	Cloudy.
27	7	0	47,5	48,5	30,28		NNE	1	Fine.
	2	0	51,0	50,5	30,22		N by W	1	Fine.
28	7	0	42,0	42,0	30,09		N by E	1	Fine.
	2	0	57,0	54,5	29,96		N by E	1	Fine.
29	7	0	47,0	45,5	29,91		NNE	1	Fine.
	2	0	59,0	55,5	29,89		NNE	1	Fine.
30	7	0	44,0	46,5	29,92		NE	1	Cloudy.
	2	0	61,5	57,0	29,87		NNE	1	Cloudy.

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## METEOROLOGICAL JOURNAL

for May 1781.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points.	Str.	
	H. M.			Inches.	Inch.			
May	1	7 0	48,0	52,5	29,96	NNW	1	Cloudy.
		2 0	61,0	68,5	29,96	NE	1	Fine.
	2	7 0	46,0	49,5	30,05	SE	1	Fine.
		2 0	62,0	59,0	30,05	SE	1	Fine.
	3	7 0	48,5	49,0	30,01	SE	1	Fine.
		2 0	54,5	55,0	29,98	ESE	1	Rainy.
	4	7 0	46,5	50,0	29,93	NE	1	Cloudy.
		2 0	53,0	56,0	29,93	NE	1	Rainy.
	5	7 0	43,5	46,5	30,04	NE	1	Cloudy.
		2 0	52,0	51,5	30,06	NE	1	Fine.
	6	7 0	43,0	44,0	30,17	ESE	1	Fine.
		2 0	51,0	50,0	30,16	ENE	1	Cloudy.
	7	7 0	43,5	44,0	30,14	ENE	1	Fine.
		2 0	52,0	49,0	30,12	SE	1	Fine.
	8	7 0	42,0	44,0	29,97	NE	1	Cloudy.
		2 0	51,0	49,5	29,88	ESE	1	Fine.
	9	7 0	43,0	47,0	29,72	NNW	1	Fine.
		2 0	56,0	51,0	29,68	NE	1	Fine.
	10	7 0	42,5	46,0	29,66	NE	1	Fine.
		2 0	59,0	51,0	29,58	ESE	1	Fine.
	11	7 0	44,5	49,0	29,62	ESE	1	Rainy.
		2 0	63,0	67,5	29,73	SSW	1	Cloudy.
	12	7 0	58,5	55,5	30,01	SE	1	Fine.
		2 0	72,0	64,0	30,02	SE	1	Fine.
	13	7 0	62,0	61,0	29,91	NW	1	Cloudy.
		2 0	73,5	69,5	29,92	NW	1	Fine.
	14	7 0	56,0	63,0	29,89	ESE	1	Cloudy.
		2 0	71,5	68,5	29,89	SSE	1	Fine.
	15	7 0	56,5	51,5	29,93	NNW	1	Cloudy.
		2 0	67,0	68,0	29,96	NW	1	Fine.
	16	7 0	52,0	54,0	29,96	SE	1	Cloudy.
		2 0	57,0	61,0	30,00	NE	1	Cloudy.

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## METEOROLOGICAL JOURNAL

for May 1781.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
May 17	7	0	50,0	57,0	30,09	0,050	NE	1	Cloudy.
	2	0	57,0	59,5	30,08		NE	1	Cloudy.
18	7	0	48,5	53,5	29,95	0,114	ENE	1	Rainy.
	2	0	57,5	58,0	29,95		NE	1	Rainy.
19	7	0	55,0	56,0	29,93	0,076	NNW	1	Cloudy.
	2	0	60,5	60,5	29,93		ENE	1	Cloudy.
20	7	0	57,0	57,0	29,89	0,099	ENE	1	Cloudy.
	2	0	64,5	62,0	29,93		NE	1	Fine.
21	7	0	57,0	57,0	30,05		NE	1	Fine.
	2	0	69,0	67,0	30,08		E by N	1	Fine.
22	7	0	50,0	54,0	30,29		NE	1	Cloudy.
	2	0	57,5	58,5	30,29		NE	2	Fine.
23	7	0	57,0	48,0	30,35		NE	2	Fine.
	2	0	58,0	57,0	30,35		NE	2	Fine.
24	7	0	49,0	49,0	30,36		NE	2	Fine.
	2	0	56,0	55,0	30,34		SE	1	Fine.
25	7	0	51,5	50,0	30,23		SW	1	Fine.
	2	0	61,0	58,0	30,22		SW	1	Fine.
26	7	0	50,5	52,0	30,16		ESE	1	Fine.
	2	0	65,0	61,0	30,14		ESE	1	Fine.
27	7	0	48,5	51,0	30,15		ESE	1	Fine.
	2	0	70,0	63,0	30,15		S by W	1	Fine.
28	7	0	53,0	56,5	30,22		SW	1	Fine.
	2	0	72,0	63,5	30,22		SW	1	Fine.
29	7	0	55,5	57,5	30,26		ESE	1	Fine.
	2	0	76,0	70,0	30,16		SE	1	Fine.
30	7	0	60,5	65,0	29,98		SE	1	Fine.
	2	0	80,5	71,5	29,96		SE	1	Fine.
31	7	0	63,0	69,0	30,12		SE	1	Fine.
	2	0	78,0	76,0	30,04		SE	1	Fine.

## METEOROLOGICAL JOURNAL

for June 1781.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
June 1	7	0	65,0	66,0	30,04		NE	1	Fine.
	2	0	83,5	78,5	30,03		SW	1	Fine.
2	7	0	67,0	71,0	29,98		SW	1	Fine.
	2	0	84,0	78,0	29,96		SW	1	Fine.
3	7	0	60,0	65,0	29,87		SW	1	Rainy.
	2	0	75,0	74,0	29,80		SSW	2	Showery.
4	7	0	58,5	67,5	29,82	0,169	WSW	2	Cloudy.
	2	0	70,5	70,0	29,79		SW	2	Showery.
5	7	0	60,0	55,0	29,82		S by W	1	Fair.
	2	0	75,0	68,0	29,69		SW	1	Fine.
6	7	0	59,5	56,0	29,60	0,226	SSE	1	Cloudy.
	2	0	68,0	68,5	29,54		W by S	1	Fine.
7	7	0	58,5	65,0	29,55		SSW	1	Cloudy.
	2	0	64,5	67,0	29,55		SW	1	Fair.
8	7	0	53,5	60,0	29,49	0,055	SW	1	Rainy.
	2	0	60,0	62,0	29,42		SW	1	Showery.
9	7	0	57,0	60,0	29,68		SW	1	Fine.
	2	0	64,0	64,0	29,72		SW	1	Showery.
10	7	0	58,5	59,5	29,72	0,064	E by N	1	Fine.
	2	0	60,0	65,0	29,79		SE	1	Showery.
11	7	0	59,5	61,0	29,84		SSW	1	Cloudy.
	2	0	70,5	67,0	29,79		SSE	1	Cloudy.
12	7	0	56,0	63,0	29,82	0,057	NE	1	Rainy.
	2	0	67,0	66,5	29,78		NE	1	Fine.
13	7	0	59,5	64,0	29,77		SE	1	Cloudy.
	2	0	73,5	68,5	29,74		SSE	1	Fine.
14	7	0	63,0	65,0	29,71		SSE	1	Cloudy.
	2	0	66,0	67,0	29,66		SW	1	Fair.
15	7	0	59,5	61,0	29,79		SSE	1	Fine.
	2	0	75,0	68,5	29,65		SSE	1	Fine.
16	7	0	61,0	61,5	29,78		SSW	1	Fine.
	2	0	70,5	69,0	29,78		SSE	1	Fine.

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for June 1781.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
June 17	7	0	61,5	65,0	29,84		SW	1	Fine.
	2	0	71,0	68,5	29,84		SSW	1	Fair.
18	7	0	65,5	66,0	29,91		SSE	1	Cloudy.
	2	0	73,5	70,5	29,95		SSE	1	Fair.
19	7	0	64,5	67,0	29,91	0,019	ESE	1	Rainy.
	2	0	75,0	72,5	29,84		E by S	1	S how.
20	7	0	70,0	71,5	29,82	0,098	NE	1	Fine.
	2	0	80,0	79,5	29,84		ESE	1	Fine.
21	7	0	68,0	70,5	29,79		ESE	1	Fine.
	2	0	79,0	76,0	29,70		ESE	1	Fair.
22	7	0	57,5	69,0	29,76		NE	1	Cloudy.
	2	0	75,0	73,0	29,74		NE	1	Fine.
23	7	0	57,0	64,0	29,82		NE	1	Cloudy.
	2	0	62,5	65,0	29,83		N by E	1	Rainy.
24	7	0	55,0	60,0	29,87		NE	1	Cloudy.
	2	0	67,0	66,0	29,85		NE	1	Fair.
25	7	0	55,0	61,0	29,82		NE	1	Cloudy.
	2	0	63,0	64,0	29,80		SSE	1	Cloudy.
26	7	0	59,0	62,0	29,93		NW	1	Cloudy.
	2	0	70,0	68,0	29,97		NW	1	Fine.
27	7	0	60,0	60,0	30,05		NW	1	Fine.
	2	0	73,0	70,0	30,05		NW	1	Fine.
28	7	0	63,5	66,5	30,14		NW	1	Fine.
	2	0	74,0	71,0	30,19		NNW	1	Fine.
29	7	0	59,0	59,0	30,42		NW	1	Fine.
	2	0	77,0	71,5	30,39		NW	1	Fine.
30	7	0	66,0	69,0	30,24		SW	1	Fine.
	2	0	81,0	78,0	30,24		W SW	1	Fine.

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		Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
		H.	M.			Inches.	Inch.	Points.	Str.	
July	1	7	0	65,0	67,0	30,04		SSE	1	Fine.
		2	0	85,0	75,0	29,90		SSW	1	Fine.
	2	7	0	74,0	75,0	29,69		SSW	1	Fine.
		2	0	76,0	76,0	29,77		SSW	1	Fine.
	3	7	0	65,0	69,0	29,82		SSW	1	Cloudy.
		2	0	74,0	73,0	29,85		SSW	1	Fine.
	4	7	0	63,0	65,0	30,04		SSW	1	Fine.
		2	0	71,5	71,0	30,11		WSW	2	Fine.
	5	7	0	61,0	62,0	30,25		SSE	1	Fine.
		2	0	74,5	71,5	30,22		SW	1	Fine.
	6	7	0	65,0	65,0	30,03		SE	1	Cloudy.
		2	0	78,0	74,5	29,95		SE	1	Fine.
	7	7	0	62,0	68,0	29,81	0,050	SE	1	Cloudy.
		2	0	73,0	71,0	29,78		SE	1	Cloudy.
	8	7	0	60,0	61,0	29,71	0,132	SE	2	Cloudy.
		2	0	65,5	67,5	29,74		S by W	2	Rainy.
	9	7	0	60,5	66,0	29,92	0,293	SW	1	Rainy.
		2	0	68,5	68,0	29,92		NW	2	Cloudy.
	10	7	0	68,0	65,0	29,97	0,114	SSW	1	Rainy.
		2	0	69,0	67,0	29,88		SW	1	Cloudy.
	11	7	0	59,0	60,5	29,86	0,243	WSW	1	Cloudy.
		2	0	68,0	67,5	29,93		NW	1	Cloudy.
	12	7	0	64,5	67,0	30,07		SW	1	Cloudy.
		2	0	76,5	71,0	30,13		SW	1	Fine.
	13	7	0	67,0	70,0	30,16		SW	1	Fine.
		2	0	74,5	73,0	30,16		SW	2	Cloudy.
	14	7	0	62,0	67,0	30,03		SW	1	Cloudy.
		2	0	66,5	70,0	30,01		WSW	1	Rainy.
	15	7	0	57,0	66,0	30,11		WSW	1	Fine.
		2	0	68,0	67,5	30,13		W by S	1	Fine.
	16	7	0	58,5	64,0	30,29		WSW	1	Fine.
		2	0	73,0	69,0	30,33		NW	1	Fine.

M E T E -

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
July 17	7	0	59,0	53,5	30,33		NW	1	Fine.
	2	0	73,0	69,0	30,29		NW	1	Fine.
18	7	0	62,5	67,0	30,26		SW	1	Fine.
	2	0	73,5	70,5	30,24		SW	1	Fine.
19	7	0	62,0	66,0	30,34		NW	1	Fine.
	2	0	70,0	69,0	30,35		NW	1	Fine.
20	7	0	62,0	65,0	30,41		NW	1	Fine.
	2	0	69,0	68,5	30,41		NW	1	Fine.
21	7	0	61,0	64,0	30,44		ESE	1	Fine.
	2	0	72,0	70,0	30,41		NW	1	Fine.
22	7	0	61,0	63,0	30,33		NW	1	Fine.
	2	0	75,0	72,0	30,23		NW	1	Fine.
23	7	0	62,0	65,0	30,17		NNW	1	Cloudy.
	2	0	72,5	71,0	30,15		NW	1	Fine.
24	7	0	64,0	69,0	30,07		SSW	1	Cloudy.
	2	0	79,0	75,0	30,03		WSW	1	Fine.
25	7	0	65,0	70,0	29,96		WSW	1	Fine.
	2	0	77,5	74,0	29,93		SSW	1	Fine.
26	7	0	60,0	65,0	29,89		SW	1	Cloudy.
	2	0	71,0	70,0	30,01		SW	1	Cloudy.
27	7	0	59,0	67,0	30,06		NNW	1	Cloudy.
	2	0	73,0	70,0	30,06		NNW	1	Cloudy.
28	7	0	73,0	67,0	30,05		NW	1	Fine.
	2	0	75,0	71,0	30,01		SW	1	Fine.
29	7	0	66,0	68,0	30,01	0,213	SW	1	Cloudy.
	2	0	76,0	73,0	30,08		SW	1	Fine.
30	7	0	68,0	71,0	30,19		SW	1	Cloudy.
	2	0	80,0	76,0	30,17		SW	1	Fine.
31	7	0	67,0	70,0	30,06		SE	1	Fine.
	2	0	84,0	80,0	30,05		SE	1	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	68,5	71,0	29,96		WSW	1	Cloudy.
	2	0	78,0	75,0	30,01		SW	1	Fine.
2	7	0	63,0	65,0	30,12		SW	1	Fine.
	2	0	71,5	69,5	30,11		WSW	1	Fine.
3	7	0	63,0	64,0	30,27		SW	1	Fine.
	2	0	73,0	70,0	30,28		SW	1	Fine.
4	7	0	63,0	65,0	30,39		SW	1	Cloudy.
	2	0	73,0	70,0	30,37		NE	1	Fine.
5	7	0	62,0	64,0	30,33		NE	1	Fine.
	2	0	72,0	70,0	30,24		NE	1	Fine.
6	7	0	63,0	65,5	30,07		SSE	1	Cloudy.
	2	0	77,0	72,0	30,07		SE	1	Fine.
7	7	0	63,0	66,0	30,08		SE	1	Cloudy.
	2	0	77,0	72,0	30,07		SE	1	Fine.
8	7	0	62,0	68,0	29,94	0,026	WSW	1	Cloudy.
	2	0	66,0	68,5	29,91		SW	1	Rainy.
9	7	0	63,0	67,0	29,95	0,684	SSW	1	Fair.
	2	0	78,0	71,5	29,98		SSW	1	Fine.
10	7	0	68,5	70,5	29,90	0,042	SSW	1	Cloudy.
	2	0	80,0	76,0	30,03		SSW	1	Cloudy.
11	7	0	63,0	71,5	30,06		SSW	1	Fine.
	2	0	82,0	77,0	30,05		SE	1	Fine.
12	7	0	68,0	71,0	30,02		SSW	1	Fine.
	2	0	81,5	78,0	30,01		SW	1	Fine.
13	7	0	67,0	72,0	29,93		SW	1	Rainy.
	2	0	80,5	75,5	29,87		SW	1	Fine.
14	7	0	63,0	72,5	29,93		SW	1	Fine.
	2	0	75,0	74,0	29,85		SW	1	Cloudy.
15	7	0	58,0	68,5	29,65	0,271	SSW	2	Fair.
	2	0	65,5	70,0	29,59		SSW	2	Rainy.
16	7	0	61,0	67,5	29,62	0,067	SW	1	Fine.
	2	0	72,0	69,5	29,61		SSW	1	Fair.

M E T E

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
			without	within.	Inches.	Inch.	Points.	dir.	
Aug. 17	7	0	60,5	65,5	29,74	0,182	SW	1	Fine.
	2	0	72,0	68,0	29,83		SW	1	Fine.
18	7	0	57,0	64,0	29,89	0,087	SW	1	Fine.
	2	0	73,0	68,5	29,86		SSW	1	Fine.
19	7	0	58,0	64,5	29,73		NNE	1	Rainy.
	2	0	59,0	65,0	29,66		N by W	1	Rainy.
20	7	0	53,0	62,5	29,77	0,147	NW	1	Fine.
	2	0	67,0	66,0	29,82		NW	1	Cloudy.
21	7	0	55,0	62,0	30,11		NW	1	Fine.
	2	0	68,0	66,0	30,15		NNW	1	Fine.
22	7	0	52,0	61,0	30,25		NE	1	Fine.
	2	0	71,0	69,5	30,27		NE	1	Fine.
23	7	0	60,0	65,0	30,04		SW	1	Cloudy.
	2	0	80,0	70,0	29,96		SW	1	Fine.
24	7	0	63,0	67,0	29,76		SE	1	Cloudy.
	2	0	75,5	71,0	29,66		SSE	1	Fine.
25	7	0	61,5	68,5	29,54	0,302	WSW	1	Fine.
	2	0	72,0	71,0	29,67		SW	1	Cloudy.
26	7	0	61,5	68,0	29,93		SW	1	Fine.
	2	0	75,5	73,0	29,96		SW	1	Cloudy.
27	7	0	68,0	69,5	29,82		SSE	1	Cloudy.
	2	0	79,0	76,0	29,75		SSW	1	Fine.
28	7	0	67,5	70,5	29,66		SSW	1	Cloudy.
	2	0	65,0	72,5	29,58		SW	1	Rainy.
29	7	0	64,0	70,0	29,59		SW	2	Fine.
	2	0	72,5	71,0	29,77		SW	2	Fine.
30	7	0	64,0	68,0	30,01		SW	1	Fine.
	2	0	74,0	70,0	29,98		SSE	1	Cloudy.
31	7	0	66,0	70,0	29,72	0,390	SE	1	Rainy.
	2	0	75,0	73,0	29,88		SE	1	Fine.



1781	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
Jan.	51,5	25 0	38,5	49,0	32,0	35,9	30,55	29,04	29,90	1,348
Feb.	52,5	31,0	42,8	51,0	36,0	45,7	30,34	28,95	29,65	1,676
Mar.	60,5	32,0	44,8	53,5	35,0	45,9	30,47	29,88	30,21	0,292
Apr.	66,5	37,0	49,2	67,0	38,0	52,8	30,34	29,29	29,88	0,650
May	80,5	42,0	56,8	76,0	44,0	56,8	30,36	29,58	30,05	0,619
June	84,0	53,5	66,2	79,5	55,0	66,8	30,42	29,42	29,84	0,688
July	84,0	57,0	68,4	80,0	53,5	68,7	30,44	29,69	30,05	1,045
Aug.	82,0	52,0	67,7	78 0	61,0	69,2	30,39	29,54	29,95	2,198
Mean of 8 mon.			53,0			55,2			29,95	8,516





## A P P E N D I X.

- I. *Account of a new Kind of Rain. Written by the Count de Gioeni, an Inhabitant of the 3d Region of Mount Etna; communicated by Sir William Hamilton, K. B. F. R. S.*  
See p. 1.

*Volat per Mare magnum cinis decoctus, et terrenis nubibus excitatis, transmarinis quoque provincias pulvereis guttis implevit.* CASSIOD. lib. IV. var. epist. 50.

THE morning of the 24th instant there appeared here a most singular phenomenon. Every place, exposed to the air, was found wet with a coloured cretaceous grey water, which, after evaporating and filtrating away, left every place covered with it to the height of two or three lines; and all the iron-work that was touched by it became rusty.

The public, inclined to the marvellous, fancied various causes of this rain, and began to fear for the animals and vegetables.

In places where rain water was used, they abstained from it: some suspecting vitriolic principles to be mixed with it, and others predicting some epidemical disorder.

Those who had observed the explosions of Etna twenty days and more before, were inclined to believe it originated from one of them.

The shower extended from N.  $\frac{1}{4}$  N.E. to S.  $\frac{1}{4}$  SW. over the fields, about seventy miles in a right line from the vertex of Etna.

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There is nothing new in volcano's having thrown up sand \*, and also stones †, by the violent expansive force generated within them, which sand has been carried by the wind to distant regions.

But the colour and subtilty of the matter occasioned doubts concerning its origin; which increased from the remarkable circumstance of the water in which it came incorporated ‡; for which reasons some other principle or origin was suspected.

It became, therefore, necessary by all means to ascertain the nature of this matter, in order to be convinced of its origin, and of the effects it might produce. This could not be done without the help of a chemical analysis. To do this then with certainty, I endeavoured to collect this rain from places where it was most probable no heterogeneous matter would be mixed with it. I therefore chose the plant called *Brassica Capitata*, which having large and turned-up leaves, they contained enough of this coloured water; many of these I emptied into a vessel, and left the contents to settle till the water became clear.

This being separated into another vessel, I tried it with vegetable alkaline liquors and mineral acids; but could observe no decomposition by either. I then evaporated the water in order to reunite the substances that might be in solution; and

\* The authority of CASSIOD. prefixed to this account is strengthened by SENECA, in his 2 lib. de Quest. Nat.

*Etna aliquando multo igne abundavit, ingentem vim arenæ urentis effudit, involutus est micis pulvere, populosque subita nox terruit.*

But without having recourse to the numerous old accounts of this volcano, and of Vesuvius, we have, within these twenty years, seen many of those rains in Sicily originating in Etna; and the last, preceding the eruption of last year, was composed of little fragments of bituminous pumice stone, or *stunie*.

† The stone, described by PLINY, which fell in Thrace, the shower of stones on mount Albano after the ruin of Alba, which LIVY mentions, and many others of like nature, remarked by the ancients as miraculous rains, have been discovered to be volcanic. As to Etna we have, in our days, seen new mountains formed by the stones, or rather the lava; and as to the ancients, besides STRABO and many others, the poet PINDAR writes, that *aliquando non tantum rivus igneus eiecit, sed saxa ignita*. PIND. ap Brit. lib. V. c. 14 p. 2.

‡ In many of the writers on Etna, showers of sand, or other productions, mixed with water, are not to be found.

touching it again with the aforesaid liquors, it shewed a slight effervescence with the acids. When tried with the syrup of violets, this became a pale green; so that I was persuaded it contained a calcareous salt\*. With the decoction of galls no precipitation was produced.

The matter being afterwards dried in the shade, it appeared a very subtile, fine earth, of a cretaceous colour, but inert, from having been diluted by the rain.

I next thought of calcining it with a slow fire, and it assumed the colour of a brick. A portion of this being put into a crucible, I applied to it a stronger heat, by which it lost almost all its acquired colour. Again, I exposed a portion of this for a longer time to a very violent heat (from which a vitrification might be expected); it remained however quite soft, and was easily bruised, but returned to its original dusky colour.

From the most accurate observations of the smoke from the three calcinations, I could not discover either colour or smell that indicated any arsenical or sulphureous mixture.

Having therefore calcined this matter in three portions, with three different degrees of fire, I presented a good magnet to each; it did not act either on the first or second; a slight attraction was visible in many places on the third: this persuaded me, that this earth contains a martial principle in a metallic form, and not in a vitriolic substance†.

The nature of these substances then being discovered, their volcanic origin appears; for iron, the more it is exposed to violent calcination, the more it is divided, by the loss of its phlogistic principle; which cannot naturally happen but in the great chimney of a volcano. Calcareous salt, being a marine salt combined with a calcareous substance by means

\* Tried likewise with a solution of lead in the vegetable acid, it lost its natural colour and its transparency, and became milky. I should be inclined to believe this to be the effect of the alkaline particles, and thus account for the efflorescence on the iron's being exposed to the air.

† Because, otherwise the water would not have produced an effervescence with the acids, but would have shewn it with the alkalies; and, in the triple calcination, the red colour would rather have been increased than diminished.

of violent heat \*, cannot be otherwise composed than in a volcano †.

As to their dreaded effects on animals and vegetables, every one knows the advantageous use, in medicine, both of the one and the other, and this in the same form as they are thus prepared in the great laboratory of nature.

Vegetables, even in flower, do not appear in the least macerated, which has formerly happened from only showers of sand ‡.

How this volcanic production came to be mixed with water may be conceived in various ways.

Etna, about its middle regions, is generally surrounded with clouds that do not always rise above its summit, which is 2900 paces § above the level of the sea. This matter being thrown out, and descending upon the clouds below it, may happen to mix and fall in rain with them in the usual way. It may also be conjectured, that the thick smoke which the volcanic matter contained might, by its rarefaction, be carried in the atmosphere by the winds, over that tract of country ||; and then, cooling so as to condense and become specifically

\* The burning of lime-stone may indeed produce the composition from whence results the calcareous salt: but it is evident, that such a quantity could only proceed from a volcano.

† Many and repeated experiments on the produce of Etna have persuaded me, that marine salt is one of the chief and most abundant *menstrua* which excite the effervescence of a volcano, or that it is the basis of it (as a friend of great knowledge has lately made me believe). I find calcareous salt in the old lava, and common salt sublimated to ammoniac in the fissures and openings of the new irruptions. But this is not the place for that which requires a larger volume. I may, perhaps, say more of it on another occasion.

‡ I have repeatedly observed, that the sand-showers of our mountain are mostly composed of calcined matter, and of little crystals of schorl, with a small portion of arsenical and sometimes saline sulphureous particles, which unites the schorl to the other substances, so that the particles or grains are thereby enlarged. Sometimes the rain falls to the ground still warm.

§ The measure of the height of the mountain has twice come out to be thus described; not, however, that I give it for certain, well knowing that altimetry requires exact instruments and repeated observations. I mean to try it with the barometer, when convenient.

|| That this hypothesis may not appear exaggerated as to the quantity of smoke that

specifically heavier than the air, might descend in that coloured rain.

I must, however, leave to philosophers (to whom the knowledge of natural agents belongs) the examination and explanation of such phenomena, confining myself to observation and chemical experiments \*.

P. S. On Friday the 4th of May, about a quarter past three in the afternoon, a slight shock of an earthquake was felt in the country about Etna, which became more sensible at some distance from the mountain: its direction was from north to south. The volcano had continued its flames and explosions; and the night before, a column of smoke, composed of globes as it were piled upon each other, had ascended over the crater to double the height of the mountain, as far at least as one could judge at the distance of twenty-two miles, which the vertex is in a right line from this city. This remained the whole night perpendicular, only one of the globes had separated and lengthened out to the westward from the summit. Now and then all the inside of the column, and of the lengthened out-part, became illuminated by electric fire, which was of a deep red colour, and gradually went out again, beginning at the bottom, in about two seconds.

that must be supposed, I shall mention what was observed by CICERO: *Cratere flamma erumpit, fumo mixta tam copioso, ut, dum Boreas spirat, Melitam usque per aerem illum sublimem propellat ad x. millia passuum spatium.* cic. de Nat. Deor. lib. II.

\* WALLERIUS (in his Mineral. vol. II. Hidrol. § 5.) says: *La Physique est plus universelle dans ses vûes, et plus philosophique dans son examen, le physicien envisage, raisonne, explique, le naturaliste regarde, ramasse, et range; celui-ci vous dira il existe tel corps dans la nature, il est fait, soit au dedans, soit au dehors de telle ou telle maniere, il est de tel ou tel regne, classe, ordre, espèce, variété; celui la pretendra vous expliquer les causes de son existence, de ses formes, et de ses propriétés.*

The illustrious LINNÆUS, in Anal. Transalp. anno 1740, § 2. says thus: *Physica est scientia de qualitatibus elementorum, historia naturalis autem circa cognitionem corporum naturalium versatur.* The true naturalist ought to be learned both in physics and in chemistry; but still we know not where the division between the two sciences is.



The fire has continued on the crater till this day, May 8th, ejecting red-hot masses or stones, which rolling beautifully down the cone, have illuminated this region; some lava has run over from the crater towards the W.N.W. but without having force enough to burst the sides or walls of the volcano; so that we may apply the historical passage, MARCO ÆMILIO C. AURELIO Coss. *Ætna mons terræmotu, ignes super verticem late diffudit.* Jul. Obsequ. de Prodig. c. 89.

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II. *Of the Method of rendering very sensible the weakest Natural or Artificial Electricity.* By Mr. Alexander Volta, Professor of Experimental Philosophy in Como, &c. &c.; communicated by the Right Hon. George Earl Cowper, F. R. S. See p. 237.

## P A R T I.

1. **I**T will be readily allowed, that an apparatus capable of rendering perceptible, or, as it were, of magnifying the smallest, and otherwise unobservable, degrees of natural as well as artificial electricity, is of great advantage to the science of electricity in general, and especially for the investigation of atmospherical electricity, which by this means may be rendered very sensible and conspicuous when it is not to be discovered by common atmospherical conductors. This method is founded upon a particular use of my *electrophorus*, which is a machine well known to electricians.

2. Whenever in observing the atmospherical electricity, no degree of it can be discovered by the ordinary methods of performing those experiments, it is difficult to determine whether any electricity at all does or does not exist in the atmosphere at those times; since it may exist, and the quantity of it only be so small as not to affect the electrometers employed. An ordinary conductor, erected in the best manner for the purpose of observing the atmospherical electricity, when the sky is free from electrical clouds, seldom or never shews any signs of electricity. In that case, therefore, if we rely upon the common electrometers, even the most sensible, we must conclude, that neither the conductor nor the atmosphere, so high as the conductor reaches, contains any electricity; but by means of the apparatus I am going to describe, it will be found, that the said conductors are never entirely void of electricity.

tricity, and it must be consequently concluded, that the air, which surrounds them, is also at all times electrified. This method not only shews the existence of electricity, but gives also room to ascertain whether it is positive or negative, and that when the atmospherical conductor itself is not capable of attracting the finest thread; but if the conductor were to shew any very small attraction, then, by means of our apparatus, there may be obtained even strong sparks.

3. The electrophorus in this case might perhaps better deserve the name of *electrometer*, or *micro-electrometer*, but I had rather call it a *condenser of electricity*, for the sake of using a word which expresses at once the reason and cause of the phenomena to be treated of in this paper, as will be made evident in the second part.

The whole method may be reduced to the following few observations. I. An electrophorus must be procured, the resinous coat of which must be very thin, and either not at all electrified, or, if electrified, its electricity be entirely extinguished.

II. Its usual metal plate must be laid upon this resinous and unelectrified plate, in full and flat contact; but care must be taken that it does in no point touch the lamina of metal upon which the resinous stratum is usually fastened.

III. Those plates being so conjointly placed, a conducting communication, *viz.* a wire must be brought from the atmospherical conductor to touch the metal plate of the electrophorus, and to touch that only.

IV. The apparatus must be left in that situation for a certain time, *viz.* till the metal plate may have acquired a sufficient quantity of electricity through the conducting communication, which brings it from the atmospherical conductor very slowly.

V. Lastly, the conducting communication must be removed from the contact of the metal plate: the metal plate is then separated from the resinous one, by lifting it up by its insulating handle, after which it is in a state of attracting, of electrifying an electrometer, or, if the electricity is sufficiently strong, of giving sparks, &c. at the same time the atmospherical conductor

conductor itself shews either no electricity at all, or exceeding small signs of it.

4. It was mentioned above (IV.) that the conducting wire must be left in contact with the metal plate *for a certain time*, the length of which, however, is not easily determined, since it depends upon variable circumstances. When the conductor itself shews no signs of electricity, then it will be necessary to leave the apparatus, as directed above, during eight, ten, or more minutes. But if the conductor itself is capable of just attracting a very small thread, then it will be sufficient to leave the apparatus in contact as above mentioned, for a few seconds only, in order afterwards to obtain from it very conspicuous electrical appearances.

5. Respecting the conducting communication between the atmospherical conductor and the metal plate, care should be taken that it be made of the fewest joints possible, or rather of one piece, since the difficulty of transmitting very small quantities of electricity is considerably increased by every interruption, and it may thereby be quite obstructed, as is often the case when a chain is used for that purpose.

6. As for the electrophorus to be used, it must be farther remarked, first, that its being very thin, as mentioned above, is of great importance; it having been observed, that the thinner the resinous stratum is, the greater quantity of electricity can be accumulated into the metal plate laid upon it; which is the case whether the electricity is brought to it from the atmosphere, as in the abovementioned instance, or from any other electric power. The thickness of one-fiftieth of an inch, or that of a common coat of varnish, is very proper; whereas if the resin was an inch thick or more, the experiments would answer very badly.

7. Secondly, the surface of the resinous stratum, as well as the under surface of the metal plate, must be as plain and as smooth as possible, in order that the two surfaces may coincide more perfectly when laid one upon the other. It is well known how much this circumstance favours the effect of the electrophorus; for this reason, in my publication on that instrument, I recommended

I recommended it as a thing essential to observe\*: but this circumstance is still more essential when the same apparatus is to serve as a *condenser* of electricity.

8. Lastly, it deserves to be repeatedly and particularly observed, that the resinous plate, when it is to be used for our experiment, should be quite free from any the least electricity, otherwise the experiments cannot be depended upon. If, therefore, the resinous plate has been excited before, so as to remain in some measure electrified, all possible care should be taken to deprive it of that electricity, which however is not easily done. The most effectual method of doing it to expose the resinous plate to the hot rays of the sun or to the fire, so that its surface may be slightly melted, by which means it will entirely lose its electricity†. The flame of a candle, or of a piece of paper, will easily deprive the resin of its electricity, if its surface be passed over the flame. In order to observe whether the resinous plate is quite free from any electricity, the metal plate must be laid upon it, there it must be touched with a finger, and afterwards, being lifted up after the usual manner, it must be presented to a fine hair; for if the hair is not attracted, you may conclude, that the resinous plate has no electricity, and consequently the apparatus is fit to be used as a *condenser* of electricity.

9. Were I asked, to what degree the electricity might be condensed, or how much the electrical phenomena could be

\* See the two letters addressed to Dr. PRIESTLEY, and published in the *Scelta d'Opusculi interessanti* of Milan for the year 1775.

† It has been believed for a long time, that to heat, and especially to melt, sulphur and resins, was sufficient to excite in them some electricity; but except the tourmalin and some other stones, which are really excited by heat alone, the resins and sulphur never become electrified by that means, except when they have by some means or other suffered any friction. The mistake, as Father BECCARIA observed, was occasioned by this, *viz.* that even the least friction of the hand, or other body, is sufficient to excite such substances in those favourable circumstances; without which friction, those substances, melted and left to cool by themselves, are so far from acquiring any electricity, that they lose every vestige of it in case they were excited before the fusion, as may be easily proved by experiment: nor ought this to appear wonderful, since fusion or a strong degree of heat renders every body a conductor of electricity.

increased

increased by this apparatus; I would answer, that it is not easy to be determined, as it depends upon various circumstances; however, *cæteris paribus*, the augmentation is greater in proportion as the body which supplies the metal plate with the electricity has a greater capacity, and is larger in proportion as the electricity is weaker. Thus we observed above (§ 2. and following) that if the atmospherical conductor has barely power enough to attract a very fine thread, it is nevertheless capable of infusing such a quantity of electricity into the metal plate of the electrophorus, as to let it not only actuate an electrometer, but even dart strong sparks. But if the electricity of the atmospherical conductor is so strong as to afford some sparks, or to let the index of the electrometer rise to five or six degrees, then the metal plate of the electrophorus, which receives the electricity from this conductor, according to our method, will certainly let the index of the electrophorus rise to the highest degree, and will give a stronger spark, yet it may be plainly perceived, that the condensation is proportionably less in this than in the other case. The reason is, because the electricity cannot be accumulated beyond the greatest degree, *viz.* when the electricity is increased so much as to be dissipated every way. Therefore, according as the electric power, which supplies the condenser, is nearest to the highest degree, the condensation is proportionably less: but in that case there is no need of a condenser, since its principal use is to collect and render sensible that small quantity of electricity, which would otherwise remain imperceptible and unobserved.

10. Whenever, therefore, the atmospherical conductor by itself gives sufficiently strong signs of electricity, then there is no occasion to use our condensing apparatus. Besides, when the electricity is strong, it often happens, that part of the electricity of the metal plate is impressed upon the resin, in which case the apparatus acts as an electrophorus, and consequently is unfit for our purpose (§ 8.).

11. In order to avoid such an inconvenience, I have thought of substituting to the resinous plate a plane, which should not be a perfect electric, or quite impervious to electricity, but which should be an imperfect conductor, such as might hinder, in a cer-

tain measure only, the free passage of the electric fluid through its substance. There are many conductors of this kind; as, for instance, a clean and dry marble slab, a plate of wood (likewise clean and very dry, or covered with a coat of varnish, or wax) and the like. The surface of those bodies does not contract any electricity, or if any electricity adheres to them, it vanishes soon, on account of their semi-conducting nature; for which reason they cannot answer the office of an electrophorus, and therefore are more fit to be used as condensers of electricity.

12. Besides the advantages above mentioned, there is another, which arises from substituting an imperfectly conducting plane to the resinous plate, namely, that the metal plate laid upon one of these does actually condense or acquire a greater quantity of electricity than when laid upon the resinous plate, or other perfect electric; for since, as was said above, § 6. the thinner the resinous stratum is, the better it answers our purpose; in the case of a varnished or waxed board, this stratum becomes exceedingly thin, and it becomes nothing when an imperfectly conducting substance is used, such as a marble slab, a very dry piece of wood, &c.

13. On the other hand, care should be taken, in choosing the above mentioned plane, that it be not too much of a conducting nature, or capable of becoming so in a very short time, it being quite necessary, that the electricity should find a considerable degree of resistance in going through its substance. In choosing, or in preparing, such a plane by drying, or otherwise, it is better to render it too near to than too far from the nature of a non-conductor. A marble slab, or a board properly dried, answers admirably well, and is preferable to any other plane: otherwise the resinous plate of an electrophorus is preferable to a common table or marble slab not prepared; for these bodies, being in some measure imbibed with moisture, conduct much better than is necessary.

14. To be more particular, I shall add, that for this purpose it is better to use a flat piece of marble, and to grind it against the metal plate, till they coincide so well as to shew a sensible cohesion between them. Afterwards the piece of marble should be exposed for several days to the heat

heat of a warmed place, such as an oven, a chimney, &c. in order to expel the moisture, and to render it quite fit for our experiments (§ 12. 13.). The marble, thus prepared, will continue dry for a considerable time, except it be long exposed to very damp air. As for the small quantity of moisture which the marble may accidentally and superficially attract, it may be removed by exposing it to the sun, or to a fire, or even by wiping it with a dry and clean cloth, previous to the performing of experiments.

15. It is not every sort of marble that will answer equally for this purpose. The old marbles, which have been long preserved in dry places, answer better than those which have been recently dug from the quarry. The difference of the species of marble is also of consequence in this business; I have found some marbles which, without any preparation, answer vastly well, whereas others will not do near so well, even when properly prepared; excepting, however, when they are preserved hot during the experiment; for, in that case, they answer better than the best pieces of marble that are not preserved hot. It is always advantageous to warm the marble previous to the experiment.

16. Instead of preparing the piece of marble by a long continued heat, it will be sufficient to give it a coat of copal varnish, or amber, or lac varnish: after which it must be kept in an oven for a short time. By this means even the worst sort of marble answers very well, even without previously warming or keeping it hot during the experiment.

17. By means of the varnish even a metal plate may be used instead of the marble. This should be first made flat by grinding it against the upper plate, and then it must be varnished, but rather thicker than when the varnish is laid upon the marble. In this case both the plates might be varnished, though it is sufficient to varnish one of them.

18. Here it may be said, that in fact we are returned to the electrophorus. This is true; and indeed the varnished metal, or marble, or wood, may be excited by a very slight friction, even sometimes by the simple laying of the metal plate upon it,



especially when they are hot; hence there is no occasion to warm them, when they are good for the purpose, lest they should be so well prepared as to be easily excited, and then act like an electrophorus.

19. However, the advantages which a varnished plate has above the common electrophorus are, 1. that the varnish is always thinner than the common resinous stratum of an electrophorus. 2. That the varnish acquires a more smooth and plain surface; hence the metal plate may be more easily, and to more advantage, adapted to it.

20. Instead of the above mentioned plane of marble or metal varnished, there may be substituted, with equal advantage, any sort of plane covered with dry and clean oil-cloth or oil-silk or sattin and other silk-stuff that is not considerably thick; which will answer very well, without requiring any more than perhaps a slight warming. The silk-stuffs answer better for this purpose than those made of cotton or wool, and these better than linen. However, by a previous drying and keeping them hot during the experiment, paper, leather, wood, ivory, bone, and every sort of imperfect conductor, may be made to answer to a certain degree.

21. If those imperfectly conducting substances were dried too much, then they would become quite electrics, and consequently useless for our purpose (as will be made appear better in the second part of this paper), excepting when they were used like resins, &c.

22. I must not omit to mention also, that the apparatus may be rendered more simple by applying the silk or other semi-conducting stratum to the upper, *viz.* to the metal plate, which is furnished with a glass handle instead of the marble or other plate, which in that case becomes useless: for in its stead a plane of any kind may be used, such as a common wooden or marble table, even not very dry, a piece of metal, a book, or other conductor, whether perfect or imperfect, it being only necessary that its surface be flat.

In fact, nothing more is requisite for our experiment than that the electricity, which tends to pass from one surface to the other,

other, should find some resistance or opposition in either of the surfaces, as will be made more evident in the second part. It is immaterial whether the non-conducting or semi-conducting stratum be laid upon one or the other of the planes, it being only necessary that they should coincide very well together, which cannot be easily obtained when a common table is used for one of the planes, which is the only reason why it is better to use two planes which have been worked flat by grinding one upon the other, and one of them varnished, &c. A single metal plate, covered with silk, with three silk strings fastened to it by way of a handle, may be conveniently used for ordinary experiments.

23. Hitherto we have considered the use of our condenser in exploring the weak atmospherical electricity, which is brought down by the atmospherical conductor\*. But this, though the principal, is not the only use to which it may be applied. It serves likewise to discover the artificial electricity when this is so weak as not to be discoverable by any other means, which happens in various cases, some of which I shall now proceed to mention.

24. A Leyden phial charged, and then discharged by touching its coated sides three or four times with the discharging rod, or the hand, seems to be quite deprived of electricity, yet

\* Here it will be proper to mention a remarkable observation, which I have made on the atmospherical electricity with the help of the condenser. The late Mr. CANTON and others affirmed that they had obtained stronger signs of electricity from their atmospherical apparatus at the time of an *aurora borealis*, than at other times; but various other philosophers doubted of the influence of electricity in that meteor, and some absolutely denied it. I myself was much in doubt about it; but at present Mr. CANTON's assertion seems to be established beyond a doubt, as I have observed by actual experiment. During the strong *aurora borealis*, which appeared in the night of the 28th of July, 1780, the light of which rising gradually from the horizon, reached the zenith at near eleven o'clock, and enlightened the heavens with a reddish light, the weather being clear and windy; our condensing apparatus being applied to an atmospherical conductor, gave fine bright sparks; whereas, at other times, that is, in clear weather, and at every hour of the day or night, the same apparatus afforded either no sparks at all, or exceedingly small ones, the reason of which was because the said conductor was not much elevated.

if you touch with the knob of it the metal plate of our condenser, when properly situated (*viz.* upon an imperfectly conducting plane, &c.) and immediately after take up the said plate, this will be found to give very conspicuous signs of electricity, which shews that the Leyden phial is not quite deprived of electricity as it appeared. But if the phial was left so far charged as just to attract a light thread, then if the metal plate were to be touched by the knob of it, even for a moment, it would afterwards, when lifted up, give a strong spark, and if then it were to be touched again by the knob of the phial, it would afford a second spark hardly smaller than the former, and thus spark after spark may be obtained for a long time, which is a very surprizing experiment.

This method of producing sparks by means of a phial, which is not charged so high as to give sparks of itself, is very convenient for various pleasing experiments; as, for instance, that of lighting the inflammable air-pistol, or lamp, contrived by me, especially when a person is provided with one of those phials, prepared after the manner recommended by Mr. TIBERIUS CAVALLO\*, which when charged may be carried in the pocket for a long time. Those phials, as they retain a sensible charge for several days, will retain an insensible one for weeks or months. I mean, by an insensible charge, such as cannot be discovered but by the help of the condenser, in which case it becomes more than sensible, and sufficient for the experiment of the inflammable air-pistol, &c.

25. Secondly, Suppose you have an electrical machine so badly in order that its conductor will not afford any spark, but will just attract a thread; then if you let this conductor touch the metal plate of the condenser, and after suffering it to continue in that situation for a few minutes, whilst the machine is kept in motion, lift up the metal plate, you will obtain from it a strong spark.

26. Thirdly, In case the electrical machine acts very well, but its conductor is so badly insulated, that it will not give any sparks, as when the conductor touches the walls

\* See his Treatise on Electricity.

of the room, or when a chain falls from it upon the table; then if you let the said conductor in that state touch the metal plate of the condenser, whilst the electrical machine is in action, the plate will afterwards give sufficiently strong signs of electricity, which shews the great power this apparatus has of drawing and condensing the electricity.

27. Fourthly, The usual way of rubbing divers bodies, and then presenting them to an electrometer in order to examine their electricity, is often insufficient, that is, it makes the experimenter believe, that a body has not acquired any electricity at all, only because the quantity of it is too small to affect an electrometer. In this case it is very advantageous to rub those bodies with the metal plate of our apparatus, which plate for this purpose must be naked; for if the plate be afterwards presented to an electrometer, this will be electrified considerably, however little electricity the rubbed bodies themselves may have acquired. The quality of this electricity, *viz.* whether it be positive or negative, may be easily ascertained, since the electricity of the metal plate must be the contrary of that acquired by the body rubbed upon it. Mr. CAVALLO also made use of this method to discover the electricity of certain bodies\*. But there is a better method, to be used in case the bodies to be examined are not easily adapted to the metal plate, which method neither Mr. CAVALLO nor others have known. This is the following. The metal plate being laid upon the imperfectly conducting plane, the body to be tried is rubbed against, or is repeatedly stroked, upon it; which done, the plate is taken up, and is examined by an electrometer. If the body tried by this method is a piece of leather, a string, a piece of cloth, or velvet, or other imperfect conductor of the like sort, the plate will be certainly found electrified, and incomparably more by this means than if it were stroked by the same bodies, whilst standing insulated in the air. In short, by either of those methods you will obtain some electricity from such bodies as could hardly be expected to give any, even when they are not very dry. Indeed, coals and metals excepted,

\* See his Treatise on Electricity, part IV. chap. vi.

every

every other body will give some electricity. I can farther say, that I have often obtained some electricity even by stroking the metal plate with my naked hand.

28. It has been questioned, whether evaporation, fermentation, &c. produced any electricity, and the investigation is of consequence for determining something certain about the atmospherical electricity. I know that various persons have attempted in vain to discover electricity in those cases. Some experiments of mine relating to this purpose had also failed; nevertheless, I entertained some hopes of succeeding, as I had for a great while imagined, that effervescence, dissolution, evaporation, &c. by disturbing the natural form and situation of the particles of bodies, ought to have increased or diminished the capacity of the bodies contiguous to those in action, and consequently ought to have occasioned in some cases a rarefaction, and in others a condensation of the electric fluid. Being persuaded of this theory, I thought that the electricity produced in those cases was not discovered, partly because of its small quantity, and partly because the insulation was almost destroyed by the vapours that rose, and I imagined, that by a greater accuracy, and by multiplying the experiments, I should some time or other discover it\*. It is about two years since, that having gradually been able to condense the electricity to a great degree by means of the above described apparatus, I again thought of repeating my old experiments about the evaporation, &c. and entertained much better hopes of discovering something new about it, almost foreseeing the event; but various occupations deferred those experiments till the months of March and April of the present year 1782, when being at Paris, in company with some members of the Royal Academy of Sciences, I at last succeeded in obtaining clear signs of electricity, nay and even the spark, from the evaporation of water, from the simple combustion of coals,

\* All these thoughts are mentioned in a Latin dissertation, printed in the year 1769, and entitled, *De vi attractiva ignis electrici, ac phenomenis inde pendentibus, ad JOHANNEM BAPTISTAM BECCARIAM, &c.*

and

and from various effervescences; as those which produce inflammable air, fixed air, and nitrous air.

29. I shall finish the first part of this paper with observing, that besides the abovementioned uses, to which our condensing apparatus may be applied, the various experiments which may be made with it throw great light upon the theory of electric atmospheres in general, of which we are going to treat in the second part.

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P A R T II.

30. The experiments related in the foregoing pages have shewn how easily a metal plate, or other conducting plain surface, when properly situated, can draw the electric fluid upon itself from a weak atmospherical electricity, from a Leyden phial, &c. so as to render its effects much more conspicuous and vigorous. It is now necessary to give an explanation of those phenomena, the theory of which will greatly facilitate the practical performance of this sort of experiments.

31. The whole matter, therefore, may be reduced to this, *viz.* that the metal plate has a much greater capacity for holding electricity in one case, *viz.* when it lies upon a proper plane (as mentioned in § 11. 12. 22.) than when it stands quite insulated, as when it is suspended in the air by its silk strings or insulating handle, or when it stands upon an insulating stand, as a thick stratum of resin or the like.

32. It is easy to comprehend, that wherever the capacity of holding electricity is greater, there the intensity of electricity

is proportionably less, *viz.* a greater quantity of electricity is in that case required, in order to raise its intensity to a given degree; so that the *capacity* is inversely as the *intensity*, by which word I mean the endeavour by which the electricity of an electrified body tends to escape from all the parts of it, to which tendency or endeavour the electrical phenomena of attraction, repulsion, and especially the degree of elevation of an electrometer, correspond.

33. That the *intensity* of electricity must be inversely proportional to the *capacity* of the body electrified, will be clearly exemplified by the following experiment. Take two metal rods of equal diameter, but one of them a foot, and the other five feet long; and let the first be electrified so high as that the index of an electrometer annexed to it may be elevated to  $60^{\circ}$ ; then let this electrified rod touch the other rod, and in that case it is evident, that the intensity of the electricity, by being parted between the two rods, will be diminished in proportion as the capacity is increased; so that the index of the electrometer, which before was elevated to  $60^{\circ}$ , will now fall to  $10^{\circ}$ , *viz.* to one-sixth of the former intensity, because now the capacity is six times greater than when the same quantity of electricity was confined to the first rod alone. For the same reason, if the said quantity of electricity was to be communicated to a rod sixty times longer, its intensity would be diminished to one degree; and, on the contrary, if the electricity of this long conductor was to be contracted into the sixtieth part of that capacity, its intensity would be increased to  $60^{\circ}$ .

34. Now not only conductors of different bulk have different capacities for holding electricity, but also the capacity of the same conductor may be increased or diminished by various circumstances, some of which have not yet been properly considered. It has been observed, that the capacity of the same conductor is increased or diminished in proportion as its surface is enlarged or contracted, as is shewn by Dr. FRANKLIN's experiment of the can and chain, and various other experiments, from which it has been concluded, that the capacity of conductors

ductors is in proportion to their surface, and not to their quantity of matter.

35. This conclusion is true, but does not comprehend the whole theory, since even the extension contributes to increase the capacity; so that of two conductors, which have equal but dissimilar surfaces, that which is the more extended in length has the greater capacity\*. In short, it appears from all the experiments hitherto made, that the capacity of conductors is in proportion not to the surfaces in general, but *to the surfaces which are free, or uninfluenced by an homologous atmosphere.*

36. But that which comes nearer to our case is, that the capacity of a conductor, which has neither its form nor surface altered, is increased when, instead of remaining quite insulated, the conductor is presented to another conductor not insulated; and this increase is more conspicuous, according as the surfaces of those conductors are larger and come nearer to each other.

When an insulated conductor is opposed or presented to another conductor whatever, I call it a *conjugate conductor*.

37. The circumstance mentioned in the preceding paragraph, which augments prodigiously the natural capacity of conductors, is that which I find to have been hitherto principally overlooked, far from any advantages having been deduced from it; but let us begin with those experiments which shew this increased capacity in the simplest manner. I take, for example, the metal plate of an electrophorus, and holding it by its insulating handle in the air, electrify it so high that the index of an electrometer annexed to it might be elevated to  $60^{\circ}$ , then lowering this metal plate by degrees towards a table or other conducting plain surface, I observe that the index of the electrometer falls gradually from  $60^{\circ}$  to  $50^{\circ}$ ,  $40^{\circ}$ ,  $30^{\circ}$ , &c. Notwithstanding this appearance, the quantity of electricity in the plate remains the same, except the said plate be brought so near the table as to occasion a transmission of the electricity from the

\* See my Dissertation on the Capacity of Conductors, published at Milan in the *Opuscoli Scelti* for the year 1778; and also in ROZIER's Journal for the ensuing year.



former to the latter; at least the quantity of electricity will remain as much the same as the dampness of the air, &c. will permit. The decrease, therefore, of intensity is owing to the increased capacity of the plate, which now is not insulated *solitary* but *conjugate*. In proof of this proposition, if the plate be removed gradually farther and farther from the table, it will be found, that the electrometer rises again to its former station, namely to 60°, excepting the loss of that quantity of electricity, which during the experiment must have been more or less imparted to the air, &c.

38. The reason of this phenomenon is easily derived from the action of electric atmospheres. The atmosphere of the metal plate, which for the present I shall suppose to be electrified positively, acts upon the table or other conductor whatever to which it is presented; so that the electric fluid of the table, agreeably to the known laws, retiring to the remoter parts of it, becomes more rare in those parts which are exposed to the metal plate, and this rarefaction becomes greater the nearer the electrified metal plate is brought to the table. If the metal plate is electrified negatively, then the contrary effects must take place. In short, the parts immersed into the sphere of action of the electrified metal plate, contract a contrary electricity, which *accidental* electricity, making in some manner a compensation for the *real* electricity of the metal plate, diminishes its intensity, as is shewn by the depression of the electrometer (§ 37.).

39. The two following experiments will throw more light upon the reciprocal action of the electric atmospheres. First, suppose two flat conductors, electrified both positively or both negatively, to be presented towards, and to be gradually brought near, each other: it will appear, by two annexed electrometers, that the nearer those two conductors come to each other, the more their intensities will increase; which shews, that either of the two *conjugate* conductors has a much less capacity now than when it was singly insulated, and out of the influence of the other. This experiment explains the reason why an electrified conductor will shew a greater intensity when

when it comes to be contracted into a smaller bulk; and also why a long extended conductor will shew a less intensity than a more compact one, supposing that their quantity of surface and of electricity is the same; because the homologous atmospheres of their parts interfere less with each other in the former than in the latter case.

40. Secondly, Let the preceding experiment be repeated with this variation only, *viz.* that one of the flat conductors be electrified positively, and the other negatively: the effects then will be just the reverse of the preceding, *viz.* the intensity of their electricities will be diminished, because their capacities are increased the nearer the conductors come to each other.

41. Let us now apply the explanation of this last experiment to the other experiment mentioned in § 38. *viz.* that of bringing the electrified metal plate towards a conducting plane which is not insulated; for as this plane acquires a contrary electricity, it follows, that the intensity of the metal plate's electricity must be diminished; hence the annexed electrometer is depressed according as the capacity of the plate is increased, and consequently the plate in that case may receive a greater quantity of electricity.

42. This matter may be rendered still more clear by insulating the conducting plane, whilst the other electrified plate is upon it, and afterwards separating them; for then both the metal plate and the conducting plane (which may be called the *inferior* plane) will be found electrified, but possessed of contrary electricities, as may be ascertained by electrometers.

43. If the inferior plane is insulated first, and then the electrified plate is brought over it, then the latter will cause an endeavour in the former to acquire a contrary electricity, which, however, the insulation prevents from taking place; hence the intensity of the electricity of the plate is not diminished, at least the electrometer will shew a very little and almost imperceptible depression, which small depression is owing to the imperfection of the insulation of the inferior plane, and to the small rarefaction and condensation of the electric fluid, which may take place in different parts of the said inferior plane.

plane. But if in this situation the inferior plane be touched so as to cut off the insulation for a moment, then it will immediately acquire the contrary electricity, and the intensity in the metal plate will be diminished.

44. If the inferior plane, instead of being insulated, were itself a non-conducting substance, then the same phenomena would happen, *viz.* the intensity of the electrified metal plate laid upon it would not be diminished. This, however, is not always the case; for if the said inferior non-conducting plane is very thin, and is laid upon a conductor, then the intensity of the electrified metal plate will be diminished, and its capacity will be increased by being laid upon the thin insulating stratum; because in that case the conducting substance, which stands under the non-conducting stratum, acquiring an electricity contrary to that of the metal plate, will diminish its intensity, &c. and then the insulating stratum will only diminish the mutual action of the two atmospheres more or less, according as it keeps them more or less asunder.

45. The intensity or electric action of the metal plate, which diminishes gradually as it is brought nearer and nearer to a conducting plane not insulated, becomes almost nothing when the plate is nearly in contact with the plane, the compensation or accidental balance being then almost perfect. Hence, if the inferior plane only opposes a small resistance to the passage of the electricity (whether such resistance is occasioned by a thin electric stratum, or by the plane's imperfect conducting nature, as is the case with dry wood, marble, &c.): that resistance joined to the interval, however small, that is between the two planes, cannot be overcome by the weak intensity of the electricity of the metal plate, which on that account will not dart any spark to the inferior plane (except its electricity were very powerful, or its edges not well rounded) and will rather retain its electricity; so that, being removed from the inferior plane, its electrometer will nearly recover its former height. Besides, the electrified plate may even come to touch the imperfectly conducting plane, and may remain in that situation for some time; in which case the intensity being reduced almost

almost to nothing, the electricity will pass to the inferior plane exceedingly slowly.

46. But the case will not be the same if, in performing this experiment, the electrified metal plate be made to touch the inferior plane *edgewise*; for then its intensity being greater than when laid flat, as appears by the electrometer, the electricity easily overcomes the small resistance, and passes to the inferior plane, even across a thin stratum\*; because the electricity of one plane is balanced by that of the other, only in proportion to the quantity of surface which they oppose to each other within a given distance: whereby when the metal plate touches the other plane in flat and ample contact its electricity is not dissipated. This apparent paradox is clearly explained by the theory of electric atmospheres.

47. What looks more like a paradox is, that neither will the touching the metal plate with a finger, or with a piece of metal, deprive it of all its electricity, whilst standing upon the proper plane; so that it generally leaves it so far electrified that, when it is afterwards separated from that plane, it will still afford a spark. Indeed this phenomenon could not be explained upon the supposition that the finger or the metals were perfect conductors. But since we do not know of any perfect conductor, the metals or the finger, oppose a resistance sufficient to retard the immediate dissipation of the electricity of the

\* This explanation, properly applied, renders evident the actions of *points* in general. Properly speaking, a pointed conductor, not insulated, when presented to an electrified body, has not in itself any particular virtue of attracting electricity. It acts only like a conductor not insulated, which does not oppose any resistance to the passage of the electric fluid. If the same conductor, instead of being pointed, was to present a globular or flat surface to the electrified body, neither would it in that case oppose a greater resistance to the passage of the electricity. But the reason why the electricity will not pass nearly so easily from the electrified body to the conductor when it is flat or globular, as when it is pointed, is because in the former case the intensity of the electricity in the electrified body is weakened by the opposed flat surface, which, acquiring the contrary electricity, compensates the diminished intensity incomparably more than a point can. It appears, therefore, that it is not the particular property of a point or of a flat surface, but the different state of the electrified body, that makes it part with its electricity easier, and from a greater distance, when a pointed conducting substance, than when a flat or globular one is presented to it.

plate,

plate, which is in that case actuated by a very small degree of intensity or endeavour of expanding; so that suppose, for instance, that the piece of metal, or the finger by touching the plate, took off so much of its electricity as to reduce the intensity of the remainder to the fiftieth part of a degree; this remaining electricity would then be almost nothing; but when the plate, by being separated from the inferior plane, has its capacity so far diminished as to render the intensity of its electricity 100 times greater, then the intensity of that remaining electricity would become of two degrees or more, *viz.* sufficient to afford a spark.

48. Hitherto we have considered in what manner the action of electric atmospheres must modify the electricity of the metal plate in its various situations. We must now consider the effects which take place when the electricity is communicated to the metal plate whilst standing upon the proper plane. The whole business having been proved in the preceding pages, it is easy to deduce the applications from it; nevertheless, it will be useful to exemplify it by an experiment. Suppose that a Leyden phial or a conductor were so weakly electrified that the intensity of its electricity was only of half a degree or even less: if the metal plate of our apparatus, when standing upon the proper plane, was to be touched with that phial or conductor, it is evident, that either of them would impart to it a quantity of its electricity, proportional to the plate's capacity, *viz.* so much of it as should make the intensity of the electricity of the plate equal to that of the electricity in the conductor or phial, suppose of half a degree; but the plate's capacity, now that it lies upon the proper plane, is above 100 times greater than if it stood insulated in the air, or, which is the same thing, it requires 100 times more electricity in order to shew the same intensity; therefore, in this case it must require upwards of 100 times more electricity from the phial or conductor. It naturally follows, that when the metal plate is afterwards removed from the proper plane, its capacity being lessened so as to remain equal to the 100th part of what it was before, the intensity of its electricity must become of 50°; since, agree-

ably

ably to the supposition, the intensity of the electricity in the phial or conductor was of half a degree.

49. A conductor that is electrified whilst it stands in full and ample contact with another proper conductor, as above specified, and is afterwards separated from it, shews the same phenomena that are exhibited by a conductor, which, after being electrified, is contracted into a smaller bulk, or contrariwise, like Dr. FRANKLIN's experiment of the can and chain (§ 35.) &c.

50. If a small quantity of electricity applied to the metal plate of the condenser enables it to give a strong spark, it may be asked, what would a great quantity of electricity do? The answer is, that it would do nothing more, because, when the electricity communicated to the metal plate is so strong as to overcome the small resistance of the inferior plane, it will be dissipated.

51. After all that has been said in the preceding pages, it may be easily understood, that if the metal plate of our condenser can receive a good share of electricity from a Leyden phial \*, or from an ample conductor, however weakly electrified; it cannot receive any considerable quantity of it from a conductor of a small capacity; for this conductor cannot give what it has not, except it were continually receiving a stream, howsoever small, of electricity, as is the case with an atmospheric conductor, or with a prime conductor of an electrical machine, which acts very poorly but continues in action. In those cases it has been observed above (§ 4. 25.) that a considerable time is required before the metal plate has acquired a sufficient quantity of electricity.

52. As an ample conductor, weakly electrified, imparts a considerable quantity of electricity to the metal plate of our

\* In my Paper on the Capacity of simple Conductors is shewn the great capacity of a Leyden phial in comparison to its bulk, just because the electricity, which is communicated to one of its surfaces, is balanced by the contrary electricity of the opposite surface. There I shew, that the capacity of 16 square inches of coated surface is equal to the capacity of a conductor made of silvered cylindrical sticks, and nearly 100 feet long, the capacity of which is so great that its spark occasions a shock considerably strong.

condenser, so that when the said metal plate is afterwards separated from its proper plane, the electricity in it appears much condensed and vigorous; so when the same metal plate contains a small quantity of electricity, and such as cannot give a spark or affect an electrometer, that electricity may be rendered very conspicuous by communicating it to another small metal plate or condenser.

Mr. CAVALLO was the first who thought of this improvement, which he derived by reasoning upon my experiments. He actually made a small metal plate not exceeding the size of a shilling: this second condenser is certainly of great use in many cases, in which the electricity is so small as not to be at all, or not clearly, observable by my method or a first condenser only, as has been evidently proved by some experiments we made together. Sometimes the usual metal plate of my condenser acquired so small a quantity of electricity, that being afterwards taken up from the inferior plane, and presented to an extremely sensible electrometer of Mr. CAVALLO's construction, it did not affect it. In this case, if the said metal plate, thus weakly electrified, was made to touch the other small plate properly situated, and that was afterwards brought near an electrometer, the electricity was then generally stronger than what would have been sufficient to ascertain its quality.

Now, if by the help of both condensers the intensity of the electricity has been augmented 1000 times, which is by no means an exaggeration, how weak must then be the electricity of the body examined? how small must that electricity be which is produced by rubbing a piece of metal with one's hand, since when this electricity is condensed by both condensers, and then is communicated to an electrometer, it can hardly affect that instrument? Yet it is sufficient to afford conviction, that the metal can be electrified by the friction of a person's hand. Some years ago, *viz.* before the discovery of our condenser, and of Mr. CAVALLO's sensible electrometer, we were very far from being able to discover such weak excitations; whereas, at present, we can observe a quantity of electricity incomparably smaller than the smallest observable at those times.

## A P P E N D I X.

IN § 28. I mentioned, that after various attempts I at last succeeded in obtaining undoubted signs of electricity from the simple evaporation of water, and from various chemical effervescences; but as this is a fact not less interesting than new, it seems proper to subjoin in this place a faithful account of the experiments made for that purpose. The first set of experiments were made at Paris, in company with Mr. LAVOISIER and Mr. DE LA PLACE, two intelligent philosophers and members of the Royal Academy of Sciences. After I had shewn them my experiments with my condenser, they, as well as myself, began to entertain hopes of succeeding in the experiments on the evaporation, &c. Accordingly Mr. LAVOISIER ordered a large condenser with a marble plane to be made. The first experiment I attempted with this instrument, in company with Mr. DE LA PLACE, proved unsuccessful; but the weather at that time was bad, the room was narrow and full of vapours, and the apparatus was not quite in proper order. Mr. DE LA PLACE and Mr. LAVOISIER repeated those experiments in the country, and then they were attended with success, which incited us to repeat and diversify the experiments, by which means the discovery was compleated; having obtained unequivocal signs of electricity from the evaporation of water, from the simple combustion of coals, and from the effervescence of iron filings in diluted vitriolic acid. This observation was made the 13th of April of the present year 1782, and the experiments were performed in the following manner. In an open garden a long metal plate was insulated, which, by means of a large iron wire, was made to communicate with the metal plate of the condenser laid upon the piece of



marble, which was kept continually warm by some lighted coals set underneath. This done, some chafing-dishes, containing burning charcoal, were placed upon the large insulated plate. The combustion of the coals was helped by a gentle wind. Some minutes after, the iron wire, by which the large insulated plate was connected with the metal plate of the condenser, was taken off; then the metal plate being removed from the marble by its insulated handle, and presented to Mr. CAVALLLO's electrometer, made the balls of it diverge with negative electricity. The experiment was repeated by placing upon the large insulated plate four vessels, containing iron filings and water, instead of the chafing-dishes: then some vitriolic acid was poured into those four vessels, sufficient to cause a vigorous effervescence, and when the strongest ebullition was going to subside, the metal plate of the condenser was removed from over the marble; and being examined, not only electrified the electrometer with negative electricity, but gave a sensible spark. At this time having tried to obtain electricity from the evaporation of water, the effects were equivocal or hardly sensible; the same thing happened a few days after, when however we obtained clear signs of electricity from those effervescences, which produce fixed and nitrous air. Those experiments were made in a large room.

One day the electricity arising from the evaporation of water seemed to be positive; but subsequent experiments, and other circumstances, indicate that such a phenomenon must be attributed to a mistake.

Once on repeating these experiments in company with Mr. LE ROY, member of the R. A. of Sciences, we could not obtain any electricity from the evaporation of water or from combustion, the weather being extremely damp; but the effervescence of iron filings and diluted vitriolic acid produced electricity enough to ascertain that it was negative, though it afforded no spark.

A short time before I left Paris I once more repeated the experiment of the effervescence of iron filings, &c. with success. This experiment was made in the laboratory of Mr. BILLAUM, an instrument-maker and lover of electricity.

The experiment on the evaporation of water, which did not answer so well at Paris, succeeded much better in London, where I bethought me of throwing water upon the lighted coals, which were kept in an insulated chafing-dish. In this manner the electricity of the evaporation never fails to electrify the chafing-dish negatively, and strongly enough for the electricity to be discovered by the simple electrometer; it will even afford a spark, if the condenser is used. The first experiment of this sort was made at Mr. BENNET's, who is a great lover of electricity, in presence of Mr. BENNET, Mr. CAVALLO, and Mr. KIRWAN, members of the Royal Society, and of Mr. WALKER, lecturer of experimental philosophy.

Another time this experiment was repeated with success at Mr. CAVALLO's, in the following manner. A small crucible, containing three or four small coals lighted, was insulated; then a spoonful of water was thrown upon the coals, and immediately after an electrometer, which communicated with the coals by means of a wire, diverged with negative electricity.

These are the experiments which I have had the opportunity to make hitherto; in relating which I must not omit to observe, that although the condensing apparatus has not been always indispensably necessary, Mr. CAVALLO's very sensible electrometer alone having been often sufficient for the purpose; yet it must be confessed, that it was the condensing apparatus which suggested these experiments, and by the help of which even the electric spark could be obtained. These experiments have just opened the way to a vast field, which deserves much farther investigation. It is natural to suppose, that if compact bodies, when they are rarefied or become an elastic fluid, require an additional quantity of electric fluid, and consequently leave those bodies, with which they are connected, negatively electrified; it must happen, on the contrary, that when vapours condense they must part with some electric fluid, that is, must produce a positive electricity. This, however, remains to be proved experimentally, and I have already imagined several ways of trying it, which will be put in practice as soon as I shall have the opportunity. Mean while I beg leave to conclude

clude this paper with mentioning a few ideas I entertain relating to the atmospherical electricity.

The experiments hitherto made, though not numerous, yet concur to shew, that the vapours of water, and in general the parts of all bodies, that are separated by volatilization, carry away an additional quantity of electric fluid as well as of elementary heat, and consequently that those bodies, from the contact of which the volatile particles have been separated, remain both cooled and electrified negatively; from which it may be deduced, that whenever bodies are resolved into volatile elastic fluid, their capacity for holding electric fluid is augmented, as well as their capacity for holding common fire, or the caloric fluid. This is a striking analogy by which the science of electricity throws some light upon the theory of heat, and alternately derives light from it; I mean on the doctrine of latent or specific heat, the first notions of which were suggested by the admirable experiments of Dr. BLACK and WILKE, and which has been afterwards much elucidated by Dr. CRAWFORD, who followed the experiments of Dr. IRWIN.

By following this analogy it seems, that as the vapours on their condensing, lose part of their latent heat, on account of their capacity being diminished, so they part with some electric fluid. Hence originates the positive electricity, which is always more or less predominant in the atmosphere, when the sky is clear, *viz.* at that height where the vapours begin to be condensed. Accordingly, the atmospherical electricity is stronger in fogs, in which case the vapours are more condensed, so as to be almost reduced into drops, and is still stronger when thick fogs become clouds.

Hitherto we have accounted for the positive atmospherical electricity; but it is easy to account for clouds negatively electrified; for when a cloud, positively electrified, has been once formed, its sphere of action is extended a great way round, so that if another cloud comes within that sphere, its electric fluid, agreeably to the well known laws of electric atmospheres, must retire to the parts of it which are the remotest from the first cloud; and from thence the electric fluid may be communicated

nicated to other clouds, or vapours, or terrestrial prominencies. Thus a cloud may be electrified negatively, which cloud, after the same manner, may occasion a positive electricity in another cloud, &c. This explains not only the negative electricity, which is often obtained from the atmosphere in cloudy weather; and the frequent changes from positive to negative electricity, and contrariwise in stormy weather; but also the waving motion often observed in the clouds, and the hanging down of them, so as nearly to touch the earth.

After the fore-mentioned discoveries we need no longer wonder at the appearance of lightnings in the eruptions of volcanos, as was particularly observed in the late dreadful eruption of Mount Vesuvius. The few experiments I have made shew, that the quantity of smoke, but much more the rapidity with which it is produced, tends to increase the electricity which arises from combustion, &c. How great must then be the quantity of electricity that is produced in such eruptions?



## AMENDMENT to P. 191. L. 12.

I have lately repeated this experiment, and found that one measure of alkaline air is saturated by less than half of one measure of fixed air, but more than one-third, conformably to Dr. PRIESTLEY's first experiment, p. 293.; by which it appears, that 100 gr. of alkaline air require about 120 of fixed air to saturate them: and hence 100 gr. of concrete volatile alkali contain about 53 of fixed air, 44 of mere volatile alkali, and 3 of water.

# E R R A T A.

## V O L. LXXI.

Page. Line.

9. 20. *for* 0,03 *read* 0,035  
 11. 26. *for* 3,100 *read* 3106.  
 18. 12. *for*  $\frac{3,55}{0,290}$  *read*  $\frac{3,55}{0,299}$

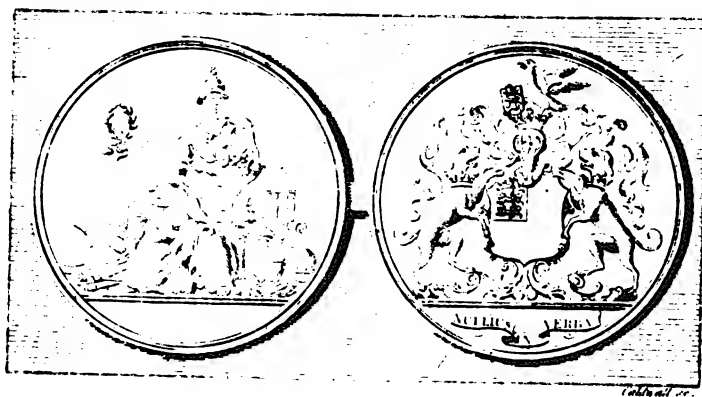
## V O L. LXXII.

73. 12. *for* one fortieth *read* one twentieth  
 75. 1. *for* five sevenths *read* five fourths  
 78. 9. *for* sevenths of an inch *read* fourths of an inch  
 81. 2. *for* now *read* more  
 117. 11. *for* 38 *read* 39  
 127. 8. *for* 108 *read* 107  
 136. 24. *for*  $\zeta$  Capricorni *read* FL. 64. Sagittarii  
 147. 3. *for* the  $1\frac{1}{2}$  *read* the farthest  $1\frac{1}{2}$   
 149. 28. *for* 40' *read* 40''  
 153. 7. *for*  $\pi$  FL. 10. *read*  $\epsilon$  FL. 11.  
 155. 4. *for* 5 *read* 4  
 161. 21. *for* 6'' 27' *read* 6'' 27'''  
 182. at the top of the 6th column, insert Physical specific gravity.  
 — first series of the 6th column, *for* 1,846 *read* 1,8846  
 183. at the top of the 6th column, insert Physical specific gravity.  
 185. 12. *for* 1,4653 *read* 1,4650  
 206. 14. *after* nitrous *read* test  
 217. 23. and 26. *for* 1 PR. *read* 4 PR.

PHILOSOPHICAL  
TRANSACTIONS,  
OF THE  
ROYAL SOCIETY  
OF  
L O N D O N.

V O L. LXXII. For the Year 1782.

P A R T II.



L O N D O N,

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PHILOSOPHICAL  
TRANSACTIONS.

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XIX. *An Attempt to make a Thermometer for measuring the higher Degrees of Heat, from a red Heat up to the strongest that Vessels made of Clay can support. By Josiah Wedgwood; communicated by Sir Joseph Banks, Bart. P. R. S.*

Read May 9, 1782.

**A** MEASURE for the higher degrees of heat, such as the common thermometers afford for the lower ones, would be an important acquisition, both to the philosopher and the practical artist. The latter must feel the want of such a measure

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on many occasions; particularly when he attempts to follow, or apply to use, the curious experiments of Mr. PORT, related in his *Lithogeognosia*, and other modern writers upon similar subjects. When we are told, for instance, that such and such materials were changed by fire into a fine white, yellow, green, or other coloured glass: and find, that these effects do not happen, unless a particular degree of fire has fortunately been hit upon, which degree we cannot be sure of succeeding in again:—when we are disappointed, by having the result at some times an unvitrified mass, and at others an over-vitrified scoria, from a little deficiency or excess of heat:—when we see colours altered, not only in shade but in kind, and in many cases destroyed, by a small augmentation of the heat which had produced them; insomuch, that in the gradual increase of the fire, a precise moment of time must be happily seized, in order to catch them in perfection:—and when inconveniences, similar to these, arise in operations by fire upon metals and other substances:—how much is it to be wished, that the authors had been able to convey to us a measure of the heat made use of in their valuable processes!

In a long course of experiments, for the improvement of the manufacture I am engaged in, some of my greatest difficulties and perplexities have arisen from not being able to ascertain the heat to which the experiment-pieces had been exposed. A *red*, *bright red*, and *white heat*, are indeterminate expressions; and even though the three stages were sufficiently distinct from each other, they are of too great latitude; as the brightness or luminousness of fire increases, with its force, through numerous gradations, which can neither be expressed in words, nor discriminated by the eye. Having no other resource, I have  
been

been obliged to content myself with such measures as my own kilns and the different parts of them afforded. Thus the kiln in which our glazed ware is fired furnishes three measures, the bottom being of one heat, the middle of a greater, and the top still greater: the kiln in which the biscuit ware is fired furnishes three or four others, of higher degrees of heat; and by these I have marked my registered experiments. But though these measures had been fully adequate to my own views, which they were not, it is plain, that they could not be communicated to others; that their use is confined to a particular structure of furnaces, and mode of firing; and that, upon any alteration in these, they would become useless and unintelligible, even where now they are best known. And, indeed, as this part of the operation is performed by workmen of the lowest class, we cannot depend upon any great accuracy even in one and the same furnace. It has accordingly often happened, that the pieces fired in the top of the kiln in one experiment have been made no hotter than those fired in the middle in another, and *vice versa*.

The force of fire, in its higher as well as lower stages, can no otherwise be justly ascertained than by its effects upon some known body. Its effect in changing colours has already been hinted at; and I have observed compositions of calces of iron with clay to assume, from different degrees of fire, such a number of distinct colours and shades as promised to afford useful criteria of the respective degrees.

With this idea, I prepared a quantity of such a composition, and formed it into circular pieces, about an inch in diameter, and a quarter of an inch thick. A number of these was placed in a kiln, in which the fire was gradually augmented, with as

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much

much uniformity and regularity as possible, for near sixty hours. The pieces, taken out at equal intervals of time during this successive increase of heat, and piled in their order upon one another in a glass tube, exhibited a regular and pretty extensive series of colours; from a flesh-colour to a deep brownish-red, from thence to a chocolate, and so on to nearly black, with all the intermediate tints between these colours. A back being fixed to the tube, like the scale of a thermometer, and the numbers of the pieces marked upon it respectively opposite to them; it is obvious, that these numbers may be considered as so many thermometric divisions or degrees; and that, if another piece of the same composition be fired in any other kiln or furnace, not exceeding the utmost heat of the first, it will acquire a colour corresponding to some of the pieces in the tube, and thus point out the degree of heat which that piece, and consequently such other matters as were in the fire along with it, have undergone.

It must however be confessed, that, for general use, a thermometer on this principle is liable to objection, as ideas of colours are not perfectly communicable by words; nor are all eyes, or all lights, equally adapted for distinguishing them, especially the shades which approach near to one another; and the effects of phlogistic vapours, in altering the colour, may not in all cases be easily guarded against.

In considering this subject attentively, another property of argillaceous bodies occurred to me; a property which obtains, in a greater or less degree, in every kind of them that has come under my examination, so that it may be deemed a distinguishing character of this order of earths: I mean, the *diminution of their bulk by fire*; I have the satisfaction to find, in a course

of experiments lately made with this view, that it is a more accurate and extensive measure of heat than the different shades of colour.

I have found, that this diminution begins to take place in a low red-heat; and that it proceeds regularly, as the heat increases, till the clay becomes vitrified, and consequently to the utmost degree that crucibles, or other vessels made of this material, can support. The total contraction of some good clays which I have examined, in the strongest of my own fires, is considerably more than one-fourth part in every dimension.

If, therefore, we can procure at all times a clay sufficiently apyrous or unvitrescible, and always of the same quality in regard to contraction by heat; and if we can find means of measuring this contraction with ease and minute accuracy, I flatter myself, that we shall be furnished with a measure of fire sufficient for every purpose of experiment or business.

We have, in different parts of England, immense beds of clay; each of which, at equal depths, is pretty uniform in quality throughout its whole extent. Of all the sorts I have hitherto tried, some of the purest Cornish porcelain clays seem the best adapted, both for supporting the intensity, and measuring the degrees, of fire.

For preparing and applying this material to thermometric purposes the following method is proposed.

The clay is first to be washed over, and, whilst in a dilute state, passed through a fine lawn. Let it then be made dry, and put up in boxes \*,

\* While the clay is thus kept dry in boxes, as well as while it continues in its natural bed, it is secure from alterations in quality, which clays in general are subject to undergo, when exposed, for a long course of years, to the joint actions of air and moisture.—In the lawns I made use of, the interstices were each less than the 100,000 part of an inch.



The dry clay is to be softened, for use, with about two fifths of its weight of water ; and formed into small pieces, in little moulds of metal, six-tenths of an inch in breadth, with the sides pretty exactly parallel, this being the dimension intended to be measured, about four-tenths of an inch deep, and one inch long. To make the clay deliver easily, it will be necessary to oil the mould, and make it warm.

These pieces, when perfectly dry, are put into another iron mould or gage, consisting only of a bottom, with two sides, five-tenths of an inch deep ; to the dimensions of which sides the breadth of the pieces is to be pared down.

For measuring the diminution which they are to suffer from the action of fire, another gage is made, of two pieces of brass, twenty-four inches long, with the sides exactly straight, divided into inches and tenths, fixed five tenths of an inch asunder at one end, and three-tenths at the other, upon a brass plate ; so that one of the thermometric pieces, when pared down in the iron gage, will just fit to the wider end. Let us suppose this piece to have diminished in the fire one-fifth of its bulk, it will then pass on to half the length of the gage ; if diminished two-fifths, it will go on to the narrowest end ; and in any intermediate degree of contraction, if the piece be slid along till it rests against the converging sides, the degree at which it stops will be the measure of its contraction, and consequently of the degree of heat it has undergone.

These are the outlines of what appears to me necessary for the making and using of this thermometer ; and it is hoped, that the whole process will be found sufficiently simple, and easy of execution. It may, nevertheless, be proper to take notice of a few minuter circumstances, and to mention  
some

some observations which occurred in the progress of the inquiry.

I. There ought to be a certainty of the clay being easily, and at all times, procurable in sufficient quantity, and on moderate terms. That this is the case with the clay here made choice of, will be evident to every one acquainted with the natural history of Cornwall, where there are beds of this clay, inexhaustible, and in too many hands to be monopolized. If this should not prove satisfactory, the author offers to this illustrious Society, and will think himself honoured by their acceptance of, a sufficient space in a bed of this clay to supply the world with thermometer-pieces for numerous ages; and he does not apprehend, that any greater inconveniences can arise to foreign artists or philosophers, from their being supplied with clay for these thermometers from this spot only, than what we now feel from being supplied with mercury for the common thermometers from the Spanish or Hungarian mines.

II. We ought to be assured also, that all the clay made use of for these thermometers is perfectly similar. For this purpose, it will be best to dig it out of the earth in considerable quantity at once, an extent of some square feet or yards in area, and to the depth of six or seven yards or more from the surface, and to mix the whole thoroughly together, previous to the further preparation already mentioned. When the first quantity is exhausted, another perpendicular column may be dug from the same bed, close to the first, to the same depth, and prepared in the same manner; by which means we may be assured of its similarity with the former parcel, and that it will diminish equally in the fire.

III. This clay, dried by the summer heat, or in a moderately warm room, or with more heat before a fire, has not been observed to differ in *degree* of dryness. After being so dried, it loses about a hundredth part of its weight in the heat of boiling water, about as much more in that of melted lead, and from thence to a red-heat ten parts, in all  $\frac{1}{1000}$ . Each of these heats soon expels from the clay its determinate quantity of matter, chiefly air; after which, the same heat, though continued for many hours, has no further effect. I had some hopes, that the graduation of the common thermometer might be continued, upon this principle, up to the red-heat at which the shrinking of the clay commences, so as to connect the two thermometers together by one series of numbers; but the loss of weight appears not to be sufficiently uniform or proportional to the degree of heat to answer that purpose; for it was found to go on quicker, and bladders tied to the mouths of the vessels in which the pieces were heated, became more rapidly distended, at the commencement of redness than at any other time. From low red-heat to a strong one, such as copper melts in, the loss of weight was only about two parts in a hundred; though the difference between these two heats appears to be much greater than what the same loss corresponds to in the lower stages. After this period, the decrease of weight intirely ceased.

The vapours expelled from the clay, caught separately in the different degrees of heat, seemed, from the few trials made with them, to consist of common air mixed with fixt air. They all precipitated lime-water; that which was first extricated, exceeding weakly; the others more and more considerably; but the last not near so strongly as the air expelled from lime-stone in burning. None of them were inflammable.

IV.

IV. The thermometric pieces may be formed much more expeditiously than in the single mould, by means of an instrument used for similar purposes by the potters. It consists of a cylindrical iron vessel, with holes, in the bottom, of the form and dimensions required. The soft clay, put in the vessel, is forced by a press down through these apertures, in long rods, which may be cut while moist, or broken when dry, into pieces of convenient lengths. It was hoped, that this method would of itself have been sufficient, without the addition of the paring gage, making proper allowance, in the size of the holes, for the shrinking of the clay in drying. But it was found, that a variety of little accidents might happen to alter the shape and dimensions of the pieces, in a sensible degree, while in their soft state; so that it will be always safest to have recourse to the paring gage, for ascertaining and adjusting their breadth when perfectly dry, this being the period at which the pieces are exactly alike with regard to their future diminishing; so that if they are now reduced to the same breadth, we may be sure that they will suffer equal contractions from equal degrees of heat afterwards, whether they have been made in a mould, or by a press, or in any other way; neither is any variation in the length or thickness of these pieces of the least consequence, provided one of the dimensions, that by which they are afterwards to be measured, is made accurate to the gage.

V. It will be proper to bake the pieces, when dry, with a low red-heat, in order to give them some firmness or hardness, that they may, if necessary, be able to bear package and carriage; but more especially to prepare them for being put into an immediate heat, along with the matters they are to serve as measures to, without bursting or flying, as unburnt clay would

do. We need not be solicitous about the precise degree of heat employed in this baking, provided only that it does not exceed the lowest degree which we shall want to measure in practice; for a piece that has suffered any inferior degrees of heat, answers as well for measuring higher ones as a piece which has never been exposed to fire at all. In this part of the preparation of the pieces, it may be proper to inform the operator of a circumstance, which, though otherwise immaterial, might at first disconcert him: if the heat is not in all of them exactly equal, he will probably find, that while some have begun to shrink, others are rather enlarged in their bulk; for they all swell a little just on the approach of redness. As this is the period of the most rapid produce of air, the extension may perhaps be owing to the air having at this moment become elastic to such a degree, as to force the particles of the clay a little asunder before it obtains its own enlargement.

VI. Each division of the scale, though so large as a tenth of an inch, answers to  $\frac{1}{10}$ th part of the breadth of the little piece of clay. We might go to much greater nicety, either by making the divisions smaller, or the scale longer; but it is not apprehended, that any thing of this kind will be found necessary: and, indeed, in proceeding much further in either way, we may possibly meet with inconveniences sufficient to counterbalance the apparent additional accuracy of measurement.

VII. The divisions of this scale, like those of the common thermometers, are unavoidably arbitrary; but the method here proposed appears sufficiently commodious and easy of execution, the divisions being adjusted by measures everywhere known, and at all times obtainable: for however the inches used in different countries may differ in length, this cannot affect

affect the accuracy of the scale, provided that the proportions between the wider and narrower end of the gage are exactly as five-tenths of those inches to three-tenths, and the length 240 of the same tenths; and that the pieces in their perfectly dry state, before firing, fit precisely to the wider end. When one gage is accurately adjusted to these proportional measures, two pieces of brass should be made, one fitting exactly into one end, and the other into the other: these will serve as standards for the ready adjustment of other gages to the dimensions of the original.

By this simple method we may be assured, that thermometers on this principle, though made by different persons, and in different countries, will all be equally affected by equal degrees of heat, and all speak the same language: the utility of this last circumstance is now too well known to need being insisted on.

VIII. If a scale two feet in length should be reckoned inconvenient, it may be divided into two, of one foot each, by having three pieces of brass fixed upon the same plate; the first and second, five-tenths of an inch apart at one end, and four-tenths at the other; the second and third, four-tenths at one end, and three-tenths at the other; so that the first reaches to the 120th division, and the second from thence to the 240th.

IX. As this thermometer, like all others, can express only the heat felt by itself, the operator must be careful to expose the pieces to an equal action of the fire with the body whose heat he wants to measure by them. In kilns, ovens, reverberatories, under a muffle, and wherever the heat is pretty steady and uniform, the means of doing this are too

obvious to need being mentioned. But in a naked fire, where the heat is necessarily more fluctuating, and unequal in different parts of the fuel, some precaution will be required.

The thermometer-piece may generally be put into the crucible, along with the subject-matter of the experiment. But where the matter is of such a kind as to melt and stick to it, the piece may be previously inclosed in a little case made of crucible clay. The smallness of the pieces will admit of this being done without inconvenience, at least in any but the smallest crucibles, as the pieces themselves may be diminished to any size that may be found proper, provided only that one of the dimensions, five-tenths of an inch, be preserved, as mentioned in *Obs.* 4.

For the very smallest sort of crucibles, the case may be put in close to the crucible, so as to form as it were an addition to its bulk on the outside. If it be asked, why the case is not always thus put in by the side of the crucible? it is answered, that in judging of the heat of *large* crucibles from a thermometer-piece placed on the outside of them, we may sometimes be deceived, as the piece in its little case has been found to heat sooner than the matter in the larger vessel; but in *small* ones, as the crucible and case are nearly alike in bulk, there is little danger of error from this cause.

X. These thermometer-pieces possess some singular properties, which we could not have expected to find united in any substance whatever, and which peculiarly fit them for the purposes they are here applied to.

1. When baked by only moderate degrees of fire, though they are, like other clays, of a porous texture, and imbibe

water;

water; yet, when saturated with the water, their bulk continues exactly the same as in a dry state.

2. By very strong fire, they are changed to a porcelain or semi-vitreous texture; nevertheless, their contraction, on further augmentations of the heat, proceeds regularly as before, up to the highest degree of fire that I have been able to produce.

3. They bear sudden alternatives of heat and cold; may be dropped at once into intense fire, and, when they have received its heat, may be plunged as suddenly into cold water, without the least injury from either.

4. Even while saturated with water in their porous state, they may be thrown immediately into a white heat, without bursting or suffering any injury.

5. Sudden cooling, which alters both the bulk and texture of most bodies, does not at all affect these, at least not in any quality subservient to their thermometric uses.

6. Nor are they affected by long *continuance* in, but solely by, the *degree* of heat they are exposed to. In three minutes or less, they are perfectly penetrated by the heat which acts upon them, so as to receive the full contraction which that degree of heat is capable of producing equally with those which had undergone its action during a gradual increase of its force for many hours. Strong degrees of heat are communicated to them with more celerity than weak ones: perhaps the heat may be more readily transmitted, in proportion as the texture becomes more compact.

These facts have been ascertained by many experiments, the particulars of which are omitted, because they would swell this paper much beyond the bulk intended.

XI. The



XI. The use and accuracy of this thermometer for measuring, *after an operation*, the degree of heat which the matter has undergone, will be apparent. The foregoing properties afford means of measuring it also, easily and expeditiously, *during the operation*, so that we may know when the fire is increased to any degree previously determined upon. The piece may be taken out of the fire in any period of the process, and dropped immediately into water, so as to be fit for measuring by the gage in a few seconds of time. At the same instant, another piece may be introduced into the place of the former, to be taken out and measured in its turn; and thus alternately, till the desired degree of heat is obtained. But as the cold piece will be two or three minutes in receiving the full heat, and corresponding contraction; to avoid this loss of time, it may be proper, on some occasions, to have two or more pieces, according to convenience, put in together at first, that they may be successively cooled in water, and the degrees of heat examined at shorter intervals. It will be unnecessary to say any thing further upon precautions or procedures which the very idea of a thermometer must suggest, and in which it is not apprehended that any difficulty can occur, which every experimenter will not readily find means to obviate.

XII. It now only remains, that the language of this new thermometer be understood, and that it may be known what the heats meant by its degrees really are. For this purpose a great number of experiments has been made, from which the following results are selected.

The scale commences at a red-heat, fully visible in daylight; and the greatest heat that I have hitherto obtained in my experiments is 160°. This degree I have produced in an air-furnace about eight inches square.

Mr. ALCHORNE has been so obliging as to try the necessary experiments with the pure metals at the Tower, to ascertain at what degrees of this thermometer they go into fusion; and it appears, that Swedish copper melts at 27, silver at 28, and gold at 32.

Brass is in fusion at 21. Nevertheless, in the brass and copper foundries, the workmen carry their fires to  $140^{\circ}$  and upwards: for what purpose they so far exceed the melting heat, or whether so great an additional heat be really necessary, I have not learnt.

The welding heat of iron is from 90 to  $95^{\circ}$ ; and the greatest heat that could be produced in a common smith's forge  $125^{\circ}$ .

Cast iron was found to melt at  $130^{\circ}$ , both in a crucible in my own furnace, and at the foundry; but could not be brought into fusion in the smith's forge, though that heat is only  $5^{\circ}$  lower. The heat by which iron is run down among the fuel for casting is  $150^{\circ}$ .

As the welding state of iron is a softening or beginning fusion of the surface, it has been generally thought that cast iron would melt with much less heat than what is necessary for producing this effect upon the forged; whereas, on the contrary, cast iron appears to require, for its fusion, a heat exceeding the welding heat  $35$  or  $40^{\circ}$ , which is much more than the heat of melted copper exceeds the lowest visible redness.

Thus we find, that though the heat for melting copper is by some called a white heat, it is only  $27^{\circ}$  of this thermometer. The welding heat of iron, or  $90^{\circ}$ , is likewise a white heat; even  $130^{\circ}$ , at which cast iron is in fusion, is no more than a white heat; and so on to  $160^{\circ}$  and upwards is all a white heat still. This shews abundantly how vague such a denomination must be, and how inadequate to the purpose of giving us any clear ideas

ideas of the extent of what we have been accustomed to consider as one of the three divisions of heat in ignited bodies.

A Hessian crucible, in the iron foundry, *viz.* about  $150^{\circ}$ , melted into a slag-like substance. Soft iron nails, in a Hessian crucible in my own furnace, melted into one mass with the bottom of the crucible, at  $154^{\circ}$ : the part of the crucible above the iron was little injured.

The *fonding* heat of the glass furnaces I examined, or that by which the perfect vitrification of the materials is produced, was at one of them  $114^{\circ}$  for flint-glass, and  $124^{\circ}$  for plate-glass; at another it was only  $70^{\circ}$  for the former, which shews the inequality of heat, perhaps unknown to the workmen themselves, made use of for the same purpose. After complete vitrification, the heat is abated for some hours to 28 or  $29^{\circ}$ , which is called the *settling* heat; and this heat is sufficient for keeping the glass in fusion. The fire is afterwards increased, for working the glass, to what is called the *working* heat; and this I found, in plate-glass, to be  $57^{\circ}$ .

Delft ware is fired by a heat of 40 or  $41^{\circ}$ ; cream-coloured, or Queen's ware, by  $86^{\circ}$ ; and stone ware, called by the French *pots de grès*, by  $102^{\circ}$ : by this strong heat, it is changed to a true porcelain texture. The thermometer-pieces begin to acquire a porcelain texture about  $110^{\circ}$ .

The above degrees of heat were ascertained by thermometer-pieces fired along with the ware in the respective kilns. But this thermometer affords means of doing much more, and going further in these measures than I could at first even have expected; it will enable us to ascertain the heats by which many of the porcelains and earthen wares of distant nations and different ages have been fired: for as burnt clay, and compositions in which clay is a prevailing ingredient, suffer no  
diminution

diminution of their bulk by being re-passed through degrees of heat which they have already undergone, but are diminished by any additional heat (according to *Obs. V.*), if a fragment of them be made to fit into any part of the gage, and then fired along with a thermometer-piece till it begins to diminish, the degree at which this happens points out the heat by which it had been fired before. Of several pieces of ancient Roman and Etruscan wares, which I have examined, none appear to have undergone a greater heat than  $32^{\circ}$ , and none less than  $20^{\circ}$ ; for they all began to diminish at those or the intermediate degrees.

By means of this thermometer some interesting properties of natural bodies may likewise be discovered or more accurately determined, and the genus of the bodies ascertained. Jasper, for instance, is found to diminish in the fire, like an artificial mixture of clay and siliceous matter; granite, on the contrary, has its bulk enlarged by fire, whilst flint and quartzose stones are neither enlarged nor diminished. These experiments were made in fires between  $70$  and  $80^{\circ}$  of this thermometer. A sufficient number of facts like these, compared with each other, and with the properties of such natural or artificial bodies as we wish to find out the composition of, may lead to various discoveries, of which I have already found some promising appearances; but many more experiments are wanting to enable me to speak with that certainty and precision on these subjects which they appear to deserve.

A piece of an Etruscan vase melted completely at  $33^{\circ}$ ; pieces of some other vases and Roman ware about  $36^{\circ}$ ; Worcester china vitrified at  $94^{\circ}$ ; Mr. SPRIMONT's Chelsea china at  $105^{\circ}$ ; the Derby at  $112^{\circ}$ ; and Bow at  $121^{\circ}$ ; but Bristol china shewed no appearance of vitrification at  $135^{\circ}$ . The common fort

of Chinese porcelain does not perfectly vitrify by any fire I could produce; but began to soften about  $120^{\circ}$ , and at  $156^{\circ}$  became so soft as to sink down, and apply itself close upon a very irregular surface underneath. The true stone Nankeen, by this strong heat, does not soften in the least; nor does it even acquire a porcelain texture, the unglazed parts continuing in such a state as to imbibe water and stick to the tongue. The Dresden porcelain is more refractory than the common Chinese, but not equally so with the stone Nankeen. The cream-coloured or Queen's ware bears the same heat as the Dresden, and the body is as little affected by this intense degree of fire.

Mr. POTT says, that to melt a mixture of chalk and clay in certain proportions, which proportions appear from his tables to be equal parts, is "among the master-pieces of art." This mixture melts into a perfect glass at  $123^{\circ}$  degrees of this thermometer.

The whole of Mr. POTT's or any other experiments may, by repeating and accompanying them with these thermometric pieces, have their respective degrees of heat ascertained, and thereby be rendered more intelligible, and useful, to the reader, the experimenter, and the working artist.

I flatter myself that a field is thus opened for a new kind of thermometrical inquiries; and that we shall obtain clearer ideas with regard to the differences of the degrees of strong fire, and their corresponding effects upon natural and artificial bodies; those degrees being now rendered accurately measurable, and comparable with each other, equally with the lower degrees of heat which are the province of the common mercurial thermometer.

## APPENDIX.

## A P P E N D I X.

### ANALYSIS OF THE CLAY OF WHICH THE THERMOMETRIC PIECES ARE FORMED.

THIS clay makes no effervescence with acids. Diluted nitrous and marine acids being boiled upon it, and afterwards saturated with fixed alkali, no precipitation or turbidness appeared. It therefore contains no calcareous earth, as that earth would have been dissolved by the acids, and precipitated from them by the alkali.

Calcined with powdered charcoal, it contracted no sulphureous smell, and the acids had no more action upon it than before. It therefore contains no gypseous matter, or combination of calcareous earth with vitriolic acid; as that acid would have formed sulphur with the inflammable principle of the charcoal, and left the calcareous earth pure, or in a state of solubility by acids.

Some of the clay was calcined with an equal weight of salt of tartar, which, for the greater certainty in regard to its purity, had been run *per deliquium*, and afterwards evaporated to dryness. The calcined mixture was boiled in water, the filtered liquor slowly evaporated, and suffered to cool at intervals. No crystallization was formed: the dry salt appeared merely alkaline as at first, and deliquiated in the air; a further proof that this clay contains no gypseous matter; for the vitriolic acid would have been absorbed by the alkali, and formed vitriolated tartar, a salt which neither liquefies in the air,

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nor

nor dissolves easily in water, and which therefore would have crystallized long before the alkali became dry, or remained after its deliquiation.

A twentieth part of gypsum, ground with clay, was very distinguishable by both the foregoing processes; producing a sulphureous smell, and calcareous earth by calcination with charcoal powder; and crystals of vitriolated tartar by calcination with the same alkaline salt.

To separate the pure argillaceous part, or that matter which in all clays forms alum with the vitriolic acid, 240 grains of this clay were thoroughly moistened with oil of vitriol, boiled to dryness, and at last made nearly red-hot. The mixture was then boiled in water; the earth which remained undissolved was treated again in the same manner with vitriolic acid, and this operation repeated five or six times. The clay was diminished in the first operation about 70 grains; but less and less in the succeeding ones, and in the last scarcely two grains. The filtered liquors yielded crystals of true alum; but its quantity was not examined, as the produce of alum from aluminous earth is already sufficiently known, and the quantity of aluminous earth itself, or its proportion to the indissoluble earth, was here the object. From the 240 grains of clay there remained in one experiment 98, and in another 95 grains of indissoluble earth; so that five parts of this clay consist of three parts of pure argillaceous or alum earth, and two parts of an earth of a different kind.

With respect to the nature of this last earth, it is easier to determine negatively what it is not, than positively what it is; but ascertaining the former will be a great step towards the discovery of the latter.

That it is not calcareous, gypseous, or argillaceous, is manifest from the experiments.—It is not jasper; as this consists, in great part, of argillaceous earth, which would have been extracted by the vitriolic acid.—It is not fluor; as this, by the same acid, would have been decomposed, its own acid expelled, and a gypseous earth left.—It is not of the micaceous kind; as the peculiar aspect of these earths would readily betray them to the eye.—It is not granite; for strong fire, which granite melts in, has no effect upon this.

Nor is there any known kind of earth to which it is in any degree similar, except those of the siliceous order; and with these it perfectly agrees in all the properties, I am acquainted with, that they possess in a state of powder.

It does not vitrify or soften with pure clay, in the strongest fire I have been able to produce. Nor is it disposed to melt with the matter of Hessian crucibles; for a little of it rubbed on the inside of a crucible, and urged with strong fire, continued white, powdery, and unaltered. Thirty grains of this earth were mixed with an equal weight of dry fossil alkali, and the same quantity of a fine white quartz sand was mixed with the same proportion of the same alkali: the two mixtures were put into two small crucibles, which were surrounded with sand in a larger one, that both might be exposed to an equal heat. They both began to melt at the same time; and at about 80° of the thermometer they had formed perfect transparent glasses.

Though these properties may not, perhaps, be thought sufficient of themselves, for determining with certainty that this substance is of the siliceous kind; yet, when joined to the negative proofs, of its not belonging to any other known order  
of



of earthy bodies, they afford the fullest evidence which the nature of the subject can admit of, that the indissoluble part of this clay is truly siliceous; and consequently that the clay consists of two parts of pure siliceous earth, to three parts of pure argillaceous or aluminous earth.

XX. *An Analysis of Two Mineral Substances, viz. the Rowley-rag-stone and the Toad-stone. By William Withering, M. D.; communicated by Joseph Priestley, LL. D. F. R. S. to Sir Joseph Banks, Bart. P. R. S.*

Read May 16, 1782.

TO SIR JOSEPH BANKS, BART. P. R. S.

DEAR SIR,

Birmingham,  
Oct. 1, 1781.

I HAVE the pleasure to lay before you an analysis of two mineral substances by Dr. WITHERING of Birmingham, whose accuracy in processes of this kind will, I doubt not, give you and the members of the Royal Society great satisfaction.

It may, perhaps, throw some additional light on the subject of these fossils to inform you, that the Rowley-rag appears, by its texture before and after fusion, and also by the quantity and quality of the air which it yields in fusion, to be the same thing with the basaltes with which you have favoured me from Scotland; and that the Toad-stone, treated in the same manner, appears (after the calcareous part has been dissolved out of it) to resemble some of the species of lava, except that it yields much more air. As Dr. WITHERING has sent specimens of the fossils in their natural state, I thought it might not be amiss to present along with them the *glassy substances* into which they are reduced by fusion.

I am, with the greatest respect, &c.

J. PRIESTLEY.

TO DR. PRIESTLEY.

SIR,

Birmingham.  
March 28, 1782.

I NOW send you the results of my examination of the Toad-stone and the Rowley-rag-stone; being part of a plan which I have long since formed for a chemical analysis of all the substances that are known to exist in the earth in large quantity.

Some years ago I transmitted to the Royal Society an analysis of the different marles found in Staffordshire, which they did me the honour to insert in their Transactions; if they think these papers likewise worth their adoption, I shall send them the results of my future inquiries!

In the course of experiments which this subject has led me to, I found it convenient to form some new tables, and to enlarge some that were less completely formed before. These tables will be useful in other branches of chemical inquiry. One of them I subjoin to the present papers. The facts taken from M. MACQUER are marked with an M; those with the \* are the consequence of my own experiments.

In order to save much repetition in future, it may not be amiss to mention, once for all, a few particulars in the conduct of these processes.

1st, By *water*, is always meant water distilled in glass vessels, or by means of a large tin refrigeratory in Mr. IRWIN's method.

2dly, Only glass or china vessels are used in the liquid processes.

3dly, By a mortar I mean those excellent ones made by Mr. WEDGEWOOD; or, as will be specified at the time, a steel mortar

mortar tempered so hard that it will bear the grinding of enamel in it without discolouration.

4thly, Filtres are never employed, it being found impossible to get the quantities accurate where they are used. The powdery parts are allowed to subside until the supernatant liquor becomes clear. This sometimes requires days or weeks; but I am ignorant of a better method. By giving the vessels a circular motion round their axes, I can greatly facilitate the subsiding of the solid contents. If the separating vessels are made like a common tart-dish, with a spreading border, the liquors may be poured off very near, without disturbing the sediments.

5thly, Phlogisticated alkaly means the vegetable fixed alkaly, prepared by the deflagration of nitre and crystals of tartar dissolved in water, and boiled with Prussian blue in such quantity that it will not any longer precipitate an earth from an acid.

I remain, &c.

W. WITHERING.

## ROWLEY-RAG.

THE stone which is the subject of the following experiments forms a range of hills in the southern part of Staffordshire. The lime-stone rocks at Dudley bed up against it, and the coal comes up to the surface against the lime-stone. The highest part of the hills is near the village of Rowley. The summit has a craggy, broken appearance, and the fields on each side to a considerable distance are scattered over with large fragments of the rock, many of which are sunk in the ground. In a quarry near Dudley, where a pretty large open-

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ing has been made in order to get materials for mending the roads, the rock appears to be composed of masses of irregular rhomboidal figures: some of these masses inclose rounded pebbles of the same materials. At the distance of four, five, or six miles from the hills, as at Bilston, Willenhall, and Wednesbury, the Rag-stone is frequently found some feet below the surface in rhomboidal pieces, forming an horizontal bed of no great depth, and seldom of more than a few yards extent. Over the whole of this tract of country it is used to mend the roads, and lately has been carried to Birmingham to pave the streets. Some people sell it in powder, as a substitute for emery in cutting and polishing.

#### MORE OBVIOUS PROPERTIES.

Its appearance dark grey, with numerous minute shining crystals. When exposed to the weather gets an ochry colour on the outside; strikes fire with steel; cuts glass; melts, though not easily, under the blow-pipe. Heated in an open fire becomes magnetic, and loses about 3 in 100 of its weight.

#### EXPERIMENTS.

A. After three drams had been broken to small pieces with a hard steel hammer, upon a plate of the same metal, it was ground to an impalpable powder in one of Mr. WEDGEWOOD'S China mortars. The mortar, which had been previously weighed, lost only one-third of a grain weight during this operation.

B. This powder was repeatedly washed with pure water, so as to carry off all the finer parts, and the coarser ground again, until

until the whole was washed away. The washings were then filtered, and the powder carefully collected and dried. The water employed in the washings did not appear to have dissolved any part of the stone; for no precipitate was formed either upon the addition of mild fixed alkaly, or of silver dissolved in the nitrous acid.

C. 100 parts of this powder were put into a small matrass, and covered with marine acid: a degree of heat was excited, and a very slight effervescence took place. Water was then added, and the mixture kept boiling for half an hour. The liquor was decanted off, and more acid added, which was boiled as before. This was decanted, and the residuum washed with water until the water came off tasteless. These waters were added to the liquors before decanted. The powder had now an ash-coloured appearance, and when dried weighed  $80\frac{1}{4}$ .

To the liquors (C) phlogificated fixed alkaly was added, until no more Prussian blue was precipitated. To effect this it took one ounce, five drams, and twelve grains of the phlogificated alkaly. The precipitate, when washed and dried, weighed 47.

E. The powder of  $80\frac{1}{4}$  (C) mixed with twice its weight of fossile fixed alkaly, was put into a black lead crucible, and exposed to a red-heat for two hours. The heat was never sufficient to render the mass fluid, nor to make it adhere firmly to the crucible. The saline part was then washed away by repeated effusions of hot water. To the remaining powder marine acid was added repeatedly, and boiled as before. The powder was now perfectly edulcorated by hot water, and when dry weighed  $47\frac{1}{2}$ .

The above liquors were all added to the liquor (C), and phlogificated fixed alkaly was dropped in, until no more Prussian

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blue

blue was precipitated. To effect this, half an ounce of the alkaly was required. This precipitate weighed 19; so that the whole of the Prussian blue weighed 66. After calcination in a crucible it was reduced to  $31\frac{1}{2}$ , and was then wholly attracted by a magnet.

F. Mild fixed alkaly was now gradually added to the liquors after the separation of the Prussian blue, and a white powder was precipitated. This powder, when well washed and dried, weighed  $46\frac{1}{2}$ . After being exposed to a low red-heat for ten minutes, it weighed only  $32\frac{1}{2}$ .

G. Theedulcorated powder (E) was now perfectly white; was not acted upon either by the vitriolic, nitrous, or marine acids, but readily melted into a glass with fossile fixed alkaly; during the melting an effervescence took place.

H. The white powder (F) readily dissolved in diluted vitriolic acid, and under a slow evaporation formed crystals which had the appearance and the taste of allum.

These crystals were then reduced to powder, and boiled in alcohol. The alcohol was decanted off, but did not appear to have dissolved any part of the powder; nor did it afford any precipitate upon the addition of mild fixed alkaly.

#### C O N C L U S I O N S.

From these experiments it appears, that the Rowley-rag-stone consists of siliceous earth, clay, or earth of allum, and calx of iron. From the latter must be deducted  $11\frac{1}{2}$  for the quantity of calciform iron, found by experiment to be contained in the quantity of phlogificated alkaly made use of, and then the proportions in 100 parts of the stone will be these:

Pure siliceous earth	-	47½
Pure clay, free from fixable air		32½
Iron in a calciform state	-	20
		<hr/>
		100

From this view of the component parts of this stone, it is not improbable, that it might advantageously be used as a flux for calcareous iron ores. The makers of iron are acquainted with such ores; but never could work them to advantage, for want of a cheap and efficacious flux.

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## T O A D - S T O N E.

FROM Derbyshire; sent to me by Mr. WHITEHURST, who has so fully and so accurately described the mode of its stratification, that it is needless to enlarge upon that subject.

### M O R E O B V I O U S P R O P E R T I E S.

Of a dark brownish grey, a granulated texture; with several cavities filled with crystallized spar. It does not strike fire with steel. It melts to a black glass.

### E X P E R I M E N T S.

A. 100 parts rubbed to an extremely fine powder in a china mortar, and boiled in marine acid; the solution was decanted: the undissolved part, after proper washing and drying, weighed 71.

B. The



B. The undissolved part was rubbed with twice its weight of mild fossil alkaly, and then exposed to a red heat in a black lead crucible for one hour.

C. This mixed mass was reduced to powder, and repeatedly boiled, first in marine, afterwards in strong vitriolic acid: the residuum now weighed 56, and was perfectly white.

D. The liquors of exp. A. and C. being put all together, phlogisticated fixed alkaly was added until no further precipitation ensued. This precipitate was a Prussian blue, which, when washed and dried, weighed 56 $\frac{1}{10}$ .

After exposure to a red-heat in a crucible for forty minutes, it weighed only 29, and was wholly attracted by the magnet.

Now the 2 oz. 5 dr. and 32 gr. of phlogisticated fixed alkaly used in this experiment contain 13 gr. of calciform iron, as ascertained by a separate trial; therefore, deducting 13 from 29, we have 16 for the quantity of calciform iron obtained from the stone.

E. The earthy parts were next precipitated from the liquors by the addition of mild fossil alkaly. The precipitate, when perfectlyedulcorated and dried, weighed 29 $\frac{1}{10}$ .

F. Distilled vinegar was added to this powder, and suffered to stand in a cool place for four hours; the vinegar was poured off, and the residuum repeatedly washed with pure water. To these liquors mild fixed alkaly was added, and a white precipitate subsided, which, when washed and dried, weighed 7 $\frac{1}{10}$ .

G. To the residuum (F) dilute vitriolic acid was added: a solution took place, which solution, by evaporation and crystallization, yielded allum.

H. The part of the residuum (F) undissolved by the vitriolic acid was boiled in nitrous acid, in marine acid, and in aqua regia, without being diminished; the weight of it when dried

was

was  $7\frac{5}{8}$ . It could not be fused by the greater heat of a blow-pipe, but melted into a glass when mixed with calcareous earth.

I. The undissolved part (exp. C.) was not fusible by itself; nor was it acted on by vitriolic, nitrous, or marine acid. It melted into a glass with fossil alkaly.

K. The precipitate of  $7\frac{5}{8}$  (exp. F.) after a sufficient exposure to heat was put into an ounce of water: the next morning the water had a pellicle upon its surface, and tasted like lime-water.

## CONCLUSIONS.

Hence it appears, that 100 parts of this specimen of Toad-stone contained

C. Siliceous earth	-	-	-	56	} = $63\frac{5}{8}$
H. More ditto	-	-	-	$7\frac{5}{8}$	
D. Calciform iron	-	-	-	-	16
F. K. Calcareous earth	-	-	-	-	$7\frac{5}{8}$
G. H. Earth of allum.	-	-	-	-	$14\frac{5}{8}$
					<hr/>
					$101\frac{5}{8}$

From the addition of  $1\frac{5}{8}$  of weight it is probable, that the substances capable of uniting with fixable air were not in the specimen used fully saturated with it, as they would be after their precipitation by the mild alkaly.

Upon repeating these experiments with different portions of the Toad-stone, the quantities of the calcareous earth were found to differ a little; but nothing further appeared to invalidate the general conclusions.

A TABLE shewing the Solubility or Insolubility of certain Saline Substances in Alcöhol.

Substances.		Results.
{	Vitriolated tartar	Infusible. M.
	Glauber's salt	Infusible. M.
	Vitriolic ammoniac.	Infusible. M.
{	Vitriol of silver	Infusible. M.
	— mercury	Infusible. M.
	— copper	Infusible. M.
	— iron	Infusible. M.
{	— zinc	Infusible. *.
	Heavy spar	Infusible. *.
{	Selenite	Infusible. M.
	Allum	Infusible. *.
	Epſom ſalt	Soluble. *.
{	Nitre	Soluble. M.
	Cubic nitre	Soluble. M.
	Nitrous ammoniac.	Soluble. M.
{	Nitre of ſilver	Soluble. M.
	— mercury	Infusible. M.
	— copper	Soluble. M.
	— lead	Soluble. *.
{	Calcareous nitre	Soluble. M.
	Calcareous ſpar	Soluble. *.

Substances.		Results.
{	Digſtive falt	Soluble. M.
	Common falt	Infoluble. M.
	Sal ammoniac.	Soluble. M.
{	Luna cornea	Infoluble. M.
	Corroſ. Sublimatè	Soluble. M.
	Muria cupri	Soluble. M.
	— ferri	Soluble. M.
{	Muria calcaria	Soluble. M.
	— magnèſie	Soluble. *.
	— aluminofia	Soluble. *.
{	Soluble tartar	Soluble. *.
	Rochelle falt	Infoluble. *
	Veget. ammoniac.	Infoluble. *
{	Verdigraſ	Soluble. *.
	Sugar of lead	Soluble. *.
{	Veg. alkaly mild	Infoluble. *
	Foſſ. alkaly mild	Infoluble. *
	Vol. alkaly mild	Soluble. *.
{	Calcareous ſpar	Soluble. *.

XXI. *New Fundamental Experiments upon the Collision of Bodies.* By Mr. John Smeaton, F. R. S. in a Letter to Sir Joseph Banks, Bart. P. R. S.

Read April 18, 1782.

TO SIR JOSEPH BANKS, BART. P. R. S.

S I R,

**T**HE subjects of the inclosed tract have been the object of my consideration for many years past; and as they contain some matters that have not only been variously reasoned about, but variously concluded upon; if what is contained therein shall appear of such a nature as either to establish truth, as it appears to me; or to prompt some more able person, in reviewing the subject, to shew what links in my chain of reasoning thereon are defective, so as to establish the whole doctrine of moving bodies upon one plain consistent basis, my end will be equally answered in offering them to you, to be laid before the Royal Society, in case you shall think that the importance of the subject shall merit the same: furthermore, I hope to be forgiven, if in some parts of this paper I have expressed myself with more pointedness than I might have done, for I declare, that it was solely owing to my earnestness that the subject of mechanic motions and powers should be fully and freely investigated, and established upon grounds that shall be uncontrovertible.

I have the honour to be, &c.

Gray's-Inn,  
April 18, 1782.

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IT is universally acknowledged, that the first simple principles of science cannot be too critically examined, in order to their being firmly established; more especially those which relate to the practical and operative parts of mechanics, upon which much of the active business of mankind depends. A sentiment of this kind occasioned my tract upon *Mechanic Power*, which was published in the Philosophical Transactions, vol. LXVI. for the year 1776. What I have now to offer was intended as a supplement thereto, and the experiments were then, in part, tried; but the completion thereof was deferred at that time, partly from want of leisure; partly to avoid too great a length of the paper itself; and partly to avoid the bringing forward too many points at once. My present purpose is to shew, that the true doctrine of the *collision of bodies* hangs as it were upon the same hook, as the doctrine of the gradual generation of motion from rest, considered in that paper; that is, that whether bodies are put into gradual motion, and uniformly accelerated from rest to any given velocity; or are put in motion, in an instantaneous manner, when bodies of any kind strike one another; the motion, or sum of the motions produced, has the same relation to mechanic power therein defined, which is necessary to produce the motion desired. To prove this, and at the same time to shew some capital mistakes in principle, which have been *assumed* as indisputable truths by men of great learning, is the reason of my now pursuing the same subject.

I do not mean to point out the particular mistakes which have been made by particular men, as that would lead me into too great a length: I shall therefore content myself with observing, that the laws of collision, which have been investigated by mathematical philosophers, are principally of three kinds; *viz.* those relating to bodies perfectly *elastic*; to bodies perfectly

perfectly unelastic, and perfectly *soft*; and to bodies perfectly unelastic, and perfectly *hard*. To avoid prolixity, I shall consider in each, only the simple case of two bodies which are equal in weight or quantity of matter striking one another. Respecting those which are perfectly elastic, it is universally agreed, that when two such bodies strike one another, no motion is lost; but that in all cases, what is lost by one is acquired by the other: and hence, that if an elastic body in motion strikes another at rest, upon the stroke the former will be reduced to a state of rest, and the latter will fly off with an equal velocity.

In like manner, if a non-elastic *soft* body strikes another at rest, they neither of them remain at rest, but proceed together from the point of collision with exactly one half of the velocity that the first had before the stroke; this is also universally allowed to be true, and is fully proved by every good experiment upon the subject.

Respecting the third species of body, that is, those that are non-elastic, and yet perfectly hard; the laws of motion relating to them, as laid down by one species of philosophers, have been rejected by another; the latter alledging, that there are no such bodies to be found in nature whereon to try the experiment; but those who have laid down and assigned the doctrine that would attend the collision of bodies of this kind (if they could be found) have universally agreed, that if a non-elastic *hard* body was to strike another of the same kind at rest, that, in the same manner as is agreed concerning non-elastic soft bodies, they neither of them would remain at rest, but would in like manner proceed from the point of collision, with exactly one half of the velocity that the first had before the stroke: in short, they lay it down as a rule attending all non-elastic bodies, whether hard or soft, that the velocity after the stroke will

be the same in both, viz. *one half* of the velocity of the original striking body.

Here is therefore the assumption of a principle, which in reality is proved by no experiment, nor by any fair deduction of reason that I know of, viz. that the velocity of non-elastic *hard* bodies after the stroke must be the same as that resulting from the stroke of non-elastic *soft* bodies; and the question now is, whether it is true or not?

Here it may be very properly asked, what ill effects can result to practical men, if philosophers should reason wrong concerning the effects of what does not exist in nature, since the practical men can have no such materials to work upon, or misjudge of? But it is answered, that they who infer an equality of effects between the two sorts, may from thence be misled themselves, and in consequence mislead practical men in their reasonings and conclusions concerning the sort with which they have abundant concern, to wit, the non-elastic *soft* bodies, of which water is one, which they have much to do with in their daily practice.

Previous to the trying my experiment on mills I never had doubted the truth of the doctrine, that the same velocity resulted from the stroke of both sorts of non-elastic bodies; but the trial of those experiments made me clearly see at least the inconclusiveness, if not the falsity of that doctrine: because I found a result which I did not expect to have arisen from either sort; and for the which, when it appeared from experiment, I could see a substantial reason why it should take place in one sort, and that it was impossible that it could take place in the other; for if it did, the bodies could not have been perfectly *hard*, which would be contrary to the hypothesis. Of this deduction I have given notice in my said tract on mills, published

lished in the Philosophical Transactions, vol. LI. for the year 1759\*.

It may also be said, that since we have no bodies perfectly elastic, or perfectly unelastic and *soft*, why should we expect and bodies perfectly unelastic and *hard*? Why may not the effects be such as should result from a supposition of their being *imperfectly elastic* joined with their being *imperfectly hard*? But here I must observe, that the supposition appears to be a contradiction in terms.

We have bodies which are so nearly perfectly elastic, that the laws may be very well deduced and confirmed by them; and the same obtains with respect to non-elastic *soft* bodies; but concerning bodies of a mixed nature, which are by far the greatest number, so far as they are wanting in elasticity, they are *soft*, and *bruise*, *yield*, or *leave a mark* in collision; and so far as they are not perfectly soft they are elastic, and observe a mixture of the law relative to each; but imperfectly elastic bodies, imperfectly hard come in reality under the *same description* as the former mixed bodies: for so far as they are imperfectly hard they are soft, and either *bruise* and *yield*, or leave a mark in the stroke; and so far as they want perfect elasticity, they are non-elastic; that is to say, they are bodies imperfectly elastic, and imperfectly soft; and in fact I have never yet seen any bodies but what come under this description. It seems, therefore, that respecting the *hardness* of bodies they differ in degrees of it, in proportion as they have a greater degree of tenacity or cohesion; that is, are further removed from perfect

\* “ The effect, therefore, of overshot wheels, under the same circumstance of quantity and fall, is at a medium double to that of the undershot: and as a consequence thereof, that non-elastic bodies, when acting by their impulse or collision, communicate only a part of their original power; the other part being spent in changing their figure in consequence of the stroke.” Phil. Trans. vol. LI. p. 133.

softness,



softness, at the same time that their elastic springs, so far as they reach, are very stiff; and hence we may (by the way) conclude, that the same mechanic power that is required to change the figure in a *small degree* of those bodies that have the popular appellation of *hard bodies*, would change it in a *great degree* in those bodies that approach towards softness, by having a small degree of tenacity or cohesion. In the former kind we may rank the harder kinds of *cast iron*, and in the latter, *soft tempered clay*.

While the philosophical world was divided by the dispute about the *old* and *new opinion*, as it was called, concerning the powers of bodies in motion, in proportion to their different velocities: those who held the old opinion contending, that it was as the velocity *simply*, asked those of the new, How, upon their principles, they would get rid of the conclusions arising from the doctrine of unelastic perfectly hard bodies? They replied, They found no such bodies in nature, and therefore did not concern themselves about them. On the other hand, those of the new opinion asked those of the old, How they would account for the case of non-elastic soft bodies, where, according to them, the whole motion lost by the striking body was retained in the two after the stroke (the two bodies moving together with the half velocity), though the two non-elastic bodies had been bruised and changed their figure by the stroke; for, if no motion was lost, the change of figure must be an effect without a cause? To obviate this, those of the old opinion seriously set about proving, that the bodies might change their figure, without any loss of motion in either of the striking bodies.

Neither of these answers have appeared to me satisfactory, especially since my mill experiments: for with respect to the first, it is no proper argument to urge the impossibility of find-

ing the proper material for an experiment, in answer to a conclusion drawn from an abstract idea. On the other hand, if it can be shewn, that the figure of a body can be changed, without a *power*, then, by the same law, we might be able to make a *forge hammer* work upon a mass of soft iron, without any other power than that necessary to overcome the friction, resistance, and original *vis inertiae*, of the parts of the machine to be put in motion: for, as no progressive motion is given the mass of iron by the hammer (it being supported by the anvil), no power can be expended that way; and if none is lost to the hammer from changing the figure of the iron, which is the only effect produced, then the whole power must reside in the hammer, and it would jump back again to the place from which it fell, just in the same manner as if it fell upon a body perfectly elastic, upon which, if it did fall, the case would really happen: the power, therefore, to work the hammer would be the same, whether it fell upon an elastic or non-elastic body; an idea so very contrary to all experience, and even apprehension, of both the philosopher and vulgar artist, that I shall here leave it to its own condemnation.

As nothing, however, is so convincing to the mind as experiments obvious to the senses, I was very desirous of contriving an experiment in point; and as I saw no hopes of finding matter to make a *direct* experiment, I turned my mind towards an indirect one; so circumscribed, however, as to prove incontestably, that the result of the stroke of two non-elastic perfectly hard bodies could not be the same as would result from the collision of two soft ones; that is, if it can be *bona fide* proved, that *one half* of the original power is lost in the stroke of soft bodies by the change of figure (as was very strongly suggested by the mill experiments); then since no such loss

can

can happen in the collision of bodies perfectly hard, the result and consequence of such a stroke must be *different*.

The consequence of a stroke of bodies perfectly hard, but void of elasticity, must doubtless be different from that of bodies perfectly *elastic*: for having no spring the body at rest could not be driven off with the velocity of the striking body, for that is the consequence of the action of the spring or elastic parts between them, as will be shewn in the result of the experiments; the striking body will therefore not be stopped, and as the motion it loses must be communicated to the other, from the equality of action and re-action, they will both proceed together, with an equal velocity, as in the case of non-elastic soft bodies: the question, therefore, that remains is, what that *velocity must be*?—It must be greater than that of the non-elastic soft bodies, because there is no mechanical power lost in the stroke. It must be less than that of the striking body, because, if equal, instead of a *loss* of motion by the collision, it will be doubled. If, therefore, non-elastic soft bodies lose half their motion, or mechanical power, by change of figure in collision, and yet proceed together with half the velocity, and the non-elastic hard bodies can lose *none* in any manner whatever; then, as they must move together, their velocity must be such as to preserve the equality of the mechanic power, *unimpaired*, after the stroke the same as it was before it.

For example, let the velocity of the striking body before the stroke be 20, and its mass or quantity of matter 8; then, according to the rule deduced from the experiments in the tract on *Mechanic Power* (see exp. third and fourth) that power will be expressed by  $20 \times 20 = 400$ , which  $\times 8 = 3200$ ; and if half of it is lost in the stroke, in the case of non-elastic soft bodies, it will be reduced to 1600; which  $\div 16$  the double quantity of matter, will give 100 for the square of their velocity; the

square root of which being 10, will be the velocity of the two non-elastic soft bodies after the stroke, being just one half of the original velocity, as it is constantly found to be. But in the non-elastic hard bodies, no power being lost in the stroke, the mechanic power will remain after it, as before it, = 3200; this, in like manner, being divided by 16, the double quantity of matter, will give 200 for the square of the velocity, the square root of which is 14. 14, &c. for their velocity after the stroke, which is to 10, the velocity of the non-elastic soft bodies after the stroke, as the square root of 2 to 1; or as the diagonal of a square to its side.

It remains, therefore, now to be proved, that precisely half of the mechanic power is *lost* in the collision of non-elastic soft bodies; for which purpose my mind suggested the following reflections. In the collision of elastic bodies the effect seemingly instantaneous, is yet performed in *time*; during which time the natural springs residing in elastic bodies, and which constitute them such, are bent or forced, till the motion of the striking body is divided between itself and the body at rest; and in this state the two bodies would then proceed together, as in the case of non-elastic soft bodies; but as the springs will immediately restore themselves in an equal time, and with the same degree of *impulsive force*, wherewith they were bent in this re-action, the motion that remained in the striking body will be totally destroyed, and the total exertion of the two springs, communicated to the original resting body, will cause it to fly off with the same velocity wherewith it was struck.

Upon this idea, if we could construct a couple of bodies in such a way that they should either act as bodies perfectly elastic; or, that their springs should at pleasure be hooked up, retained, or prevented from restoring themselves, when at their extreme degree of bending; and if the bodies under these

circumstances observed the laws of collision of non-elastic soft bodies, then it would be proved, that one half of the mechanical power, residing in the striking body, would be lost in the action of collision; because the impulsive force or power of the spring in its restitution being cut off, or suspended from acting, which is equal to the impulsive force or power to bend it (and which alone has been employed to communicate motion from one body to the other), it would make it evident, that one half of the impulsive force is lost in the action, as the other half remains *locked up* in the springs. It also follows, as a *collateral circumstance*, that be the impulsive power of the springs what it may from first to last, yet as one half of the *time* of the action is by this means cut off, in this sense also it will follow, that one half of the mechanic power is destroyed; or rather, in this case, remains locked up in the springs, capable of being *re-exerted* whenever they are set at liberty, and of producing a fresh mechanical effect, equivalent to the motion or mechanical power of the two non-elastic soft bodies *after* their collision.

Hence we must infer, that the quantity of mechanical power expended in displacing the parts of non-elastic soft bodies in collision, is exactly the same as that expended in bending the springs of perfectly elastic bodies; but the difference in the ultimate effect is, that in the non-elastic soft bodies, the power taken to displace the parts will be totally lost and destroyed, as it would require an equal mechanic power to be raised a-fresh, and exerted in a contrary direction to restore the parts back again to their former places; whereas, in the case of the elastic bodies, the operation of half the mechanic power is, as observed already, only locked up and suspended, and capable of being re-exerted without a further original accession.

These ideas arose from the result of the experiments tried upon the machine described in my said tract upon Mechanic Power, and were also communicated to my very worthy and ingenious friend WILLIAM RUSSELL, Esq. F. R. S. at the same time that I shewed him those experiments in 1759; but the mode of putting this matter to a full and fair mechanical trial has since occurred; and though some rough trials, sufficient to shew the effect, were made thereon, prior to the offering the paper on mechanical power to the Society in 1776, yet the machine itself I had not leisure to complete to my satisfaction till lately; which I mention to apologize for the length of time that these speculations have taken in bringing forward.

DESCRIPTION OF THE MACHINE FOR COLLISION.

Fig. 1. shews the front of the machine as it appears at rest when fitted for use.

A is the pedestal, and AB the pillar, which supports the whole, C, D are two compound bodies of about a pound weight each, but as nearly equal in weight as may be. These bodies are alike in construction, which will be more particularly explained by fig. 2. These bodies are suspended by two white fir rods of about half an inch diameter *ef* and *gh*, being about four feet long from the point of suspension to the center of the bodies; and their suspension is upon the cross-piece II, which is mortised through, to let the rods pass with perfect freedom; and they hang upon two small plates filed to an edge on the under side, and pass through the upper part of the rods. Their centers are at *k* and *l*, and the edges being let into a little notch, on each side the mortise, the rods are at liberty to vibrate freely upon their respective points (or rather edges) of suspension, and are determined to one plain of vibration. MN is a flat arch of white wood, which may be covered with

Z z 2

paper,

paper, that the marks thereupon may be the more conspicuous.

The cross-piece II is made to project so far before the pillar, that the bodies in their vibrations may pass clear of it, without danger of striking it; and also the arch MN is brought so far forward as to leave no more than a clearance, sufficient for the rods to vibrate freely without touching it.

Fig. 2. Shews one of the compound bodies, drawn of its full size. AB is a block of wood, and about as much in breadth as it is represented in height, through a hole in which the wood rod CC passes, and is fixed therein.

DB represents a plate of lead about three-eighths of an inch thick, one on each side, screwed on by way of giving it a competent weight. *dBefg* represents the edge of a springing plate of brass, rendered elastic by hard hammering; it is about five-eighths of an inch in breadth, and about one-twentieth of an inch thick. It is fixed down upon the wooden block at its end *dB* by means of a bridge plate, whose end is shewn *hi*, and is screwed down on each side the spring plate by a screws which being relaxed the spring can be taken out at pleasure, and adjusted to its proper situation. *kl* is a light thin slip of a plate, whose under edge is cut into teeth like a fine saw or ratchet, and is attached to the spring by a pin at *k*, which passes through it, and also through a small stud rivetted into the back part of the spring, and upon which pin, as a center, it is freely moveable.

*mn* shews a small plate or stud seen edgewise raised upon the bridge plate, through an hole in which stud the ratchet passes; and the lower part of the hole is cut to a tooth shaped properly to catch the teeth of the ratchet, and retain it together with the spring at any degree to which it may be suddenly bent; and for this intent it is kept bearing gently downward, by means of a wire-spring *opq*, which is in reality double, the bearing

bearing part at *e* being semi-circular; from which branching off on each side the rod *cc*, passes to *p*, and fixes at each end into the wood at *q*. However, to clear the ratchet, which is necessarily in the middle as well as the rod, the latter is perforated; and also the block is cut away, so far as to set the main spring at *e* free of all obstacles that would prevent its play from the point B. The part *fg* is shewn thicker than the rest, by being covered with thin kid leather tight sowed on, to prevent a certain jarring that otherwise takes place on the meeting of the springs in collision.

Let us now return to fig. 1. the marks upon the arch MN are put on as follows. *qp* is an arch of a circle from the center *l*, and *qr* an arch of a circle from the center *k* intersecting each other at *s*. Now the middle line of the marks *t*, *v*, are at the same distance from the middle line at *s* that the centers *kl* are; so that when each body hangs in its own free position, without bearing against the other, the rod *ef* will cover the mark at *t*, and the rod *gb* will cover the mark at *v*. From the point S upon the arches *Sp* and *Sq* respectively, set off points at an equal and competent distance from S each way, which will give the middle of the mark *w* and *x*: and upon the arch *Sp* find a middle point between the mark *v* and *w*, which let be *y*; and on the other side, in like manner, upon the arch *Sq* find a middle point for the mark *z*; then set off the distance *Sv* or *St* from *y* each way, and from *z* each way; and from these points, drawing lines to the respective centers *l* and *k*, they will give the place and position of the marks *a*, *b*, and *c*, *d*; and thus is the machine prepared for use.

#### FOR TRIALS ON ELASTIC BODIES.

For this use take out the pins and ratchets from each respectively, and the springs being then at liberty, with a short bit

of



of stick (suppose the same size as the rods) turn aside the rod *gb* with the right-hand, carrying the body D upwards till the stick is upon the mark *w*, as suppose at *o*; there hold it, and with the left set the body C perfectly at rest; in which case the rod *ef* will be over the mark *t*; then suddenly withdraw the stick, in the direction that the rod *gb* is to follow it, and the spring of the body D, impinging upon that of the body C, they will be both bent, and also restored; and the body C will fly off, and mount till its rod *ef* covers the mark *x*; the rod of the striking body D remaining at rest upon its proper mark of rest *v*, till the body C returns, when the body D will fly off in the same manner; the two bodies thus rebounding a number of times, losing a part of their vibration each time; but so nearly is the theory of elastic bodies fulfilled hereby, that the single advantage of originally pushing the rod *gb* beyond the mark *w*, by the thickness of the stick, or its own thickness, is sufficient to carry the rod of the quiescent body C completely to its mark *x*.

There are several other experiments that may be made with this apparatus, in confirmation of the doctrine of the collision of elastic bodies; which being universally agreed upon, and well known, it is needless further to dwell upon here; but respecting the application to non-elastic soft bodies, it is far more difficult to come at a fitness of materials for this kind of experiments, than it is for those supposing perfectly elasticity. The conclusions, however, may be attained with equal certainty.

#### FOR TRIALS ON NON-ELASTIC SOFT BODIES.

For this purpose the ratchets must be applied and put in order as before described, and the springs being both put to their point of rest, let the body D be put to its mark *w* in the same

same manner as before described, and the body C to rest. The body D being let go, and striking the body C at rest, in consequence of the stroke, the springs being hooked up by the ratchets, they both move from their resting marks  $t$ ,  $v$ , respectively toward M: Now if they both moved together, and the rod  $ef$  covered the mark  $c$ , and the rod  $gb$  covered the mark  $d$  at their utmost limit, then they would truly obey the laws of non-elastic soft bodies; because their medium ascent would be to the mark  $z$ , which is just half the angle of ascent to the mark  $x$ ; but as in this piece of machinery, though the main or principle springs are hooked up, yet every part of them, and all the materials of which they are composed, and to which they are attached, have a degree, or more properly speaking, a certain compass of *elasticity*, which, as such, is perfect, and no motion lost thereby.

We must not, therefore, expect the two compound bodies after the stroke to stick together without separating, as would be the case with bodies truly non-elastic and soft; but that from the elasticity they are possessed of, they will by rebounding be separated; but that elasticity being perfect, can occasion no loss of motion to the sum of the two bodies; so that if the body C ascends as much above its mark  $c$  as the body D falls short of its mark  $d$ , then it will follow, that their medium ascent will still be to the mark  $z$ , as it ought to have been, had they been truly non-elastic soft bodies; and this, in reality, is truly the case in the experiment, as nearly as it can be discerned.

After a few vibrations, by the rubbing of the springs against one another, they are soon brought to rest; and here they would *always rest* had they been truly and properly perfect non-elastic soft bodies; but here, as in the case of these bodies, by a change of the figure and situation of the component parts, there is expended one half of the mechanical power  
of.

of the first mover, yet in this case the other half is not *lost*, but *suspended*, ready to be re-exerted whenever it is set at liberty; and that it is really and *bona fide one half* and neither more or less, appears from this uncontroverted simple principle, that the power of restitution of a perfect spring is exactly equal to the power that bends it. And this may, in a certain degree, be shewn to be fact by experiment, if there were any need of such a proof; for if, when the bodies are at rest after the last experiment, the two rods are lashed together near the bottom with a bit of thread, and then the ratchets unpinned and removed; on cutting the thread with a pair of scissars they will each of them rebound, C towards M, and D towards N; and if they rebounded respectively to  $x$  and  $y$ , the mechanical power exerted would be the same as it was after the stroke, when the mean of their two ascents was up to the mark  $z$ ; but here it is not to be expected, because not only the motion lost by the friction of the ratchets is to be deducted, because it had the effect of real non-elasticity; but also the elasticity that separated them in the stroke, which was lost in the vibrations that succeeded; neither of which hindered the mean ascent to be to  $z$ ; but yet, under all these disadvantages in the machine (if not unreasonably ill made) the rod *ef* will ascend to *d*, and *gb* to *a*: and hence I infer, as a positive truth, that in the collision of non elastic soft bodies, *one half of the mechanic power residing in the striking body is lost in the stroke.*

Respecting bodies unelastic and perfectly hard, we must infer, that since we are unavoidably led to a conclusion concerning them, which contradicts what is esteemed a truth capable of the strictest demonstration; *viz.* that the velocity of the center of gravity of no system of bodies can be changed by any collision betwixt one another, something must be assumed that involves a contradiction. This perfectly holds,

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according

according to all the established rules, both of perfectly elastic and perfectly non-elastic *soft* bodies; rules which must fail in the perfectly non-elastic *hard* bodies, if their velocity after the stroke is to the velocity of the striking body as one is to the square root of 2; for then the center of gravity of the two bodies will by the stroke acquire a velocity greater than the center of gravity the two bodies had before the stroke in that proportion, which is proved thus.

At the outset of the striking body, the center of gravity of the two bodies in our case will be exactly in the middle between the two; and when they meet it will have moved from their half distance to their point of contact, so the velocity of the center of gravity before the bodies meet will be exactly one half of the velocity of the striking body; and, therefore, if the velocity of the striking body is 2, the velocity of the center of gravity of both will be one. After the stroke, as both bodies are supposed to move in contact, the velocity of the center of gravity will be the same as that of the bodies; and as their velocity is proved to be the square root of 2, the velocity of their center of gravity will be increased from 1. to the square root of 2.; that is, from 1. to 1.414, &c.

The fair inference from these contradictory conclusions therefore is, that an unelastic hard body (perfectly so) is a repugnant idea, and contains in itself a contradiction; for to make it agree with the fair conclusions that may be drawn on each side, from clear premises, we shall be obliged to define its properties thus: that in the stroke of unelastic hard bodies they cannot *possibly lose any* mechanic power in the stroke; because no other impression is made than the communication of motion; and yet they *must lose a quantity* of mechanic power in the stroke; because, if they do not, their common center of gravity, as

above shewn, will acquire an *increase* of velocity by their stroke upon each other.

In a like manner the idea of a *perpetual motion*, perhaps, at first sight, may not appear to involve a contradiction in terms; but we shall be obliged to confess that it does, when, on examining its requisites for execution, we find we shall want bodies having the following properties; that when they are made to *ascend* against gravitation their absolute weight shall be *less*; and that when they *descend* by gravitation (through an equal space) their absolute weight shall be greater; which, according to all we know of nature, is a *repugnant* or *contradictory idea*.





XXII. *Proceedings relative to the Accident by Lightning at Heckingham.*

LETTER FROM THE BOARD OF ORDNANCE.

S I R,

**H**AVING received information that, last summer, a stroke of lightning set fire to the Poor-house at Heckingham, near Norwich, notwithstanding it was armed with *eight pointed conductors*, we request you will communicate to us such particulars relating to that fact, as may have come to your knowledge.

We are, with great respect,

S I R,

Your most obedient humble servants,

AMHERST.

CHARLES FREDERICK.

H. STRACHEY.

J. KENRICK.

Office of Ordnance,  
22d December, 1781.

Sir Jos. Banks, Bart. President of the Royal Society.

A a a 2

Extracts



Extracts from the Minutes of the Council of the Royal Society.

January 10, 1782.

THE President laid before the Council a letter to him from the Board of Ordnance, acquainting him, that the Poor-house at Heckingham, near Norwich, had been struck by lightning, notwithstanding it was armed with eight pointed conductors; and requesting him to communicate to them such particulars relating to that fact as may have come to his knowledge.

Resolved,

That Dr. Blagden and Mr. Nairne be requested to repair to Heckingham, and examine into the circumstances of the accident, and report thereon to the Council: that they engage a draughtsman, to take such drawings as may be requisite; and that the necessary expences be defrayed by the Society.

February 7, 1782.

Dr. Blagden read to the Council his and Mr. Nairne's Report of the Survey made by them of the Poor-house at Heckingham in Norfolk, in consequence of their appointment by a former Council. The said Report was ordered to be read to the Society on Thursday the 14th instant. And the President was requested to transmit it immediately afterwards to the Board of Ordnance; and to desire that they would return the drawings as soon as they should have taken copies of them, or made such other use of them as they might think necessary.

Report

Report of the Committee.

Read February 14, 1782.

To the President and Council of the Royal Society.

GENTLEMEN,

PURSUANT to your resolution, appointing us a committee to examine the House of Industry at Heckingham in Norfolk, which had been struck by lightning although it was armed with conductors, we arrived there on the 21st of January. Seven months had then elapsed since the accident, yet we had the satisfaction to learn, that no material changes had been made in the conductors or the building in that period; some laths that had been burnt, some bricks and pantiles which had been damaged or thrown down, were replaced; but we found means to procure distinct information of those repairs from the workmen who had been employed to execute them. In order to communicate a clear idea of the accident, it will be necessary to premise a general account of the building; then to represent the manner in which the conductors were applied; and, lastly, to describe the stroke of lightning, with its effects.

The general form of the building is that of the Roman letter H (see the general plan, fig. 1.), consisting of a center range (Z) and two flanks (Y and X). It stands on a gentle rising, which can by no means be termed a hill, with its front facing S. 9° W. To the western side of the west flank, and eastern side of the east flank, some lower buildings are annexed, serving as offices of different kinds; and there are two courts, one before and the other behind the house, together with some small gardens and yards on each of the flanks, in all of which stand various detached offices, as will be easily conceived from the general plan (fig. 1.).

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The body of the building, including the great house with its annexed offices, is provided with eight chimnies, the position of which is represented in the plan (fig. 1.) at the letters A, B, C, D, E, F, G, H. Of these the six first are all placed on the ridges of the roof; namely, A and B on the ridge of the west flank, C and D on the ridge of the center range, and E and F on the ridge of the east flank; but the chimney G rises from the lower part of the roof on the eastern side of the east flank; and the chimney H from the roof of an annexed office, the boiling-room, which roof is continued down from the general roof, and projects beyond it.

Both flanks (X and Y, fig. 1.) at their north and south ends are hipped off from the ridge of the roof to the eaves on each side; consequently there are eight hips, all of which are covered or *coped* with lead; the four vallies also, formed by the intersection of the center range with the two flanks (see fig. 1.) are in like manner covered with lead, which here answers the purpose of a spout. (Two of these hips are shewn in the 2d, 3d, 4th, and 11th figures at *h*, *h*, and one of the vallies at *v*, fig. 2.) These twelve strips of lead, covering the hips and the vallies (see the general plan, fig. 1.), are all separate, not having any metalline communication with one another, as the rest of the roof consists merely of pantiles, with dropping eaves.

From the south-east corner of the east flank a wall is continued eastward (see I in the 1st and 3d figures) above 26 feet in length, having a small garden at its south side, and on the north a stable built against it as a *lean-to* (K in the 1st and 4th figures); which stable is also supported on the west by the east wall of the east flank of the building (fig. 1. at *t*). The roof of the stable being like that of a shed, slopes downward from south to north (*a* and *b* in the 4th and 11th figures); it does not reach up quite

quite to the top of the wall against which it rests to the southward, but is shorter by one course of bricks on edge (see fig. 10. and 11. at *c*); and at its junction with the wall a *flashing* of lead is carried along horizontally (from *c* to *d*, fig. 11.)  $25\frac{1}{2}$  feet in length.

We conclude this general account of the building with the dimensions of its principal parts.

	Ft.	In.
Length of the center range (Z fig. 1.) to the flanks	108	9
Length of each flank (X and Y fig. 1.)	159	7
Breadth of the center range, and of each flank,	31	4
Height from the ground to the bottom of the hips ( <i>g, g</i> , fig. 3. and 4.)	about 20	6
Height from the ground to the top of the ridge ( <i>f</i> , fig. 2. and 3. <i>e</i> , fig. 4.)	about 34	0
Height of the chimnies above the ridge of the roof (as <i>E</i> , fig. 4.)	about 3	6
Length of each hip (from <i>f</i> to <i>g</i> , fig. 3. 4. and 11.)	about 27	0
Height of the wall ( <i>l</i> ) supporting the stable (fig. 3.)	16	0
Height of the eaves of the stable above the garden to the northward of it (see general plan, fig. 1.)	7	0
Length of the stable on the outside	26	2
Breadth of the stable on the outside	15	5

To all the eight chimnies which have been described we found iron rods affixed, reaching between four and five feet above the top of the chimney, pointed at the upper end, and tapering about ten inches to that point. Each rod or bar was nearly square, measuring, upon a mean, about half an inch one way, and four-tenths of an inch the other, with the angles

just rounded off. These conductors were continued down the building by a succession of similar bars of iron, in general from six to eight feet long, joined to one another by two hooks and nuts, (see fig. 12.); that is, the corresponding ends of each bar being formed into a hook bent at right-angles, the hook of the uppermost went into a hole of the lowermost, where it was fastened with a nut, and the hook of the lowermost went into a similar hole of the bar above, where it was fixed in the same manner; the length of each of these joints, from nut to nut, was about two inches.

Though there were eight of these conductors reaching above the chimnies, yet they had only four terminations below. For the conductors to the two chimnies D and E (fig. 1. and 2.) being continued toward each other along the roof, united in the valley over the lead gutter there (at L in the 1st, 2d, and 3d figures), and from that point only one conductor was continued down the valley toward the ground. In like manner the two conductors from the chimnies A and C (fig. 1.) united in the valley of the roof between them, and were carried down toward the ground as a single rod. All the three conductors from the chimnies F, G, and H, successively joined together (see M, N, fig. 1.), and only a single rod was continued from them down the lower part of the building. Lastly, the conductor from the chimney B (fig. 1.) went down single all the way, without having formed a junction with any other.

As the conductors, therefore, in their passage down the building, were thus reduced to four, we are now to shew their four terminations. And, first, that from the chimney B, being the simplest, was carried down the western side of the west flank, till it came very near the ground, when it entered a small channel of brick-work, through which it was continued under the pavement into a narrow bricked drain, leading through

through the wall of a privy (at O, fig. 1.) into which the drain discharges itself. The conductor having passed, in the drain, through the hole in the privy wall, was continued about three feet into the open space under the seat of the privy, where it terminated in air, none of the solid work being nearer its end than six inches. As this drain is constructed to receive the foul water from the yard, and one of the water-cocks is near, some moisture will most commonly be found in it; but the stones slope so rapidly at the termination of the drain in the privy, that any water which runs thither must be immediately carried off. This conductor, as well as all the others, was kept in its place near the wall in its passage down, by ring-staples driven into the wall.

The second termination we shall describe is that belonging to the chimnies F, G, H (fig. 1.). The conductor from the chimney F in passing down the roof was joined by that from the chimney G (at M, fig. 1.) and afterwards by the rod from the chimney H (at N, fig. 1.); thence the iron was continued down till it came near the bottom of the wall, where it was turned off along the pavement toward a sink (fig. 1. Q) not quite two feet distant, through the side of which it was carried; and projecting four inches into the open space of the sink, there terminated in air. The sink is built of brick, one foot nine inches deep, and two feet and a half square within; and into its fourth side is fitted an iron grate, of the same length as the side externally, and about seven inches deep, the lower part of which lies on the bare soil. Through the middle of this grate the conductor passes, resting in contact with one of the bars. From its termination to the bottom of the sink is not less than twelve inches; and the bottom, which is of brick, slopes so much, that water can never lie upon it, there being a large

drain on the further side, which leads off from the bottom of the sink.

The third termination to be investigated is that formed by the conductors from the chimnies A and C (fig. 1.). These, after joining in the valley which lies between them, ran down, as a single rod, over the lead covering the valley, passed through a hole in that lead, where it projects over as a spout, and descended in the angle formed by the intersection of the west flank with the front of the center range (S, fig. 1.). Being arrived within eight inches of the ground, it entered a narrow channel of brick-work, through which it was conveyed into a small close drain or gutter, where it terminated, with a hooked end, in contact with one of the side bricks. It touched nothing solid, therefore, in its course under the ground, but brick-work. The small drain in which it terminated was so placed as not to receive much moisture; and this drain led into the side of a grated sink (U, fig. 1.), at the bottom of which the great drain of the fore-court begins.

Of the several conductors that have been hitherto considered, the different parts of the building to which they were affixed, and their respective terminations, very accurate drawings were made on the spot; but as these conductors were more distant from the stricken end of the house than those which remain to be described, and also shewed no marks of having been affected by the storm, we thought it unnecessary to enter into a more circumstantial detail regarding them; especially as, if any further particulars should appear hereafter to be of consequence, it will at all times be easy to refer to the original drawings and notes.

We proceed now to examine the fourth termination, by which the conductors from the chimnies D and E (see fig. 1. and

and 2.), being those nearest the stricken corner, were carried under the ground. The conductor of the chimney D, from its upper point to its final termination, consisted of ten bars, into the sixth of which the conductor coming from the chimney E was fastened by its fourth bar, reckoning from the top (at L, fig. 1. and 2.). This junction was made by a hook at the lower end of that fourth bar of the conductor from the chimney E, which hook was received into a hole of the above-mentioned sixth bar, and fixed there by a nut underneath. Here was, therefore, only one hook and nut, instead of two as in the common joints. Also at the top of this sixth bar of the conductor from the chimney E, where it united with the fifth bar, only one hook and nut were employed to form the junction, the other hook appearing never to have been put into its corresponding hole. In this same sixth bar, above the hole into which the conductor from the chimney E was inserted, we found four other spare holes, which were left quite empty.

Tracing the conductor downward from this point of union, we found it descend over the lead of the valley, to the surface of which it gradually approached, till at a hole made on purpose (*m*, fig. 2.) it passed through the lead, whence it was continued down the angle formed by the intersection of the east flank with the front of the center range (*T*, fig. 1. 2. and 5.). It no where touched the wall of the building, but was kept in its place by ring-staples (*p*, *p*, fig. 3.). Being arrived within two or three inches of the ground, it entered into a channel of brick, enclosed on all sides (at *e*, fig. 5.), in which it was continued down to the arch of the great drain of the fore-court (*x*, fig. 5.); here, having passed through a hole in the haunch of the arch (*y*, fig. 5. and 7.), it was bent off from the house through the middle of the drain, and

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ultimately



ultimately terminated in contact with the bricks at the bottom of it (at *z*, fig. 5. and 7.). This conductor, therefore, in its passage downward, did not communicate, till it reached the bottom of the drain, with any thing better able to carry off electricity than masonry or timber; for the iron-staples fastening it to the wall, and the lead lining the valley, were themselves in contact with such substances only.

As this drain, then, is the real termination of the conductor, it must now be more attentively considered. It begins at the western sink of the fore-court *U* (fig. 1.); thence it is continued (*V*, *V*, fig. 1.) with a proper declivity to the eastern sink *W* (fig. 1. 5. and 6.); it then runs under the east flank of the house (*V*2, fig. 1. and 5.), and ends beyond it in the side of the cefs-pool *P* (fig. 1.). From the grating on the sink *U* to that on the sink *W* (fig. 1.) is 89 feet, and thence to the cefs-pool *P* near 69 feet; the breadth of the drain at bottom (*z*, fig. 7.) is 14 inches; its height to the spring of the arch (fig. 7.) 16 inches, and to the crown of the arch (*x*, fig. 5. and 7.) 23 inches. When we saw it, the moist filth, or sludge, at bottom (*z*, fig. 7.) was two or three inches deep; but when the court is overflowed, as the two grates (at *U* and *W*, fig. 1.) are laid on purpose to receive the superfluous moisture, there must be some run of water through it. We estimated the fall of the drain, from the eastern sink *W* (fig. 1.) to its termination in the cefs-pool *P*, at two feet. The cefs-pool itself resembles a well, walled round in the inside, and has foul water stagnating at the bottom, which cannot rise above a certain height on account of a large drain, leading from it into the great reservoir (at *R*, fig. 1.), out of which the foul water is ultimately pumped. When we examined this cefs-pool, the water in it stood even with the bottom of that great drain, consequently was almost

as high as it could be, unless the drains should at any time be flooded; and upon measuring the distance from the bottom of the drain coming from the fore-court (V2, fig. 1.) where it terminated in the side of the cefs-pool, down to the surface of the water stagnating in the cefs-pool, we found it  $3\frac{1}{2}$  feet. This interval, therefore, of three feet and a half must be passed through, to form a communication between the water in the drain, and that in the cefs-pool. The drain is firmly built of brick and mortar (see the section of it, fig. 7.). To determine the nature of the soil in which it is laid, a hole was dug in the fore-court seven feet deep, where we found nothing but sand, at this time pretty moist, with a few pebbles. There is reason to believe, however, from the soil of an adjacent declivity to the northward, that below the sand, perhaps at the depth of 15 or 16 feet, a bed of clay would be found.

Against the east flank, near the corner T (fig. 1.), there rises a leaden pipe with a cock (O, fig. 2.), to which the water is conveyed from a raised cistern (see r, fig. 1.) in one of the detached offices of the back-court. A main of lead from the cistern, which is itself of that metal, after sending out pipes to some other cocks, and passing through the cellars of the house, comes into the fore-court about four yards from the corner T (fig. 1.) and is carried over the drain at the distance of about a foot above its crown, and eight inches below the surface of the ground. Here it divides into two branches, one of which goes straight to the cock at O (fig. 2.), and the other runs westward, to supply a similar cock in the opposite corner. We measured the distance of these pipes and cocks from the conductors, and found that they came no where nearer than five feet and an half.

Such

Such were the conductors that, in the month of June, 1777, several years after the House of Industry had been built, were erected with the hope of guarding it from lightning. The iron of which they were formed had in that time acquired a coat of rust, such as might be expected from four years exposure to the air. On the 17th of June, 1781, after a showery forenoon, a heavy cloud having risen from the S. W. brought on a severe thunder-storm, attended with such heavy hail and rain, that the court before the house was overflowed. At length, about three in the afternoon, when this storm had already lasted 15 or 20 minutes, a single and very loud explosion was heard, like the report of a cannon, which exceedingly terrified all the people in the house, and affected three of the paupers so much that they fainted. At the same time a great light was perceived, which seemed, as they expressed it, to come in at the windows, and still more at the doors of the rooms, like a sheet of fire. Within one or two minutes, the south-east corner of the east flank of the building was observed to be on fire, the flame bursting out at the bottom of the hip (see g in fig. 3. 4. and 11.). By the brisk exertions of the people in the house, this fire was quickly extinguished; and the court was so overflowed, that they procured sufficient water for that purpose by means of a hole which they dug near the burning corner of the building. The storm, and especially the rain, continued some time after the stroke, but not with such violence as before. At the moment of the explosion it was nearly calm; but the wind had been south-westerly all day, and the sky was observed to be clearing in that quarter about the time of the accident.

To come at the fire, in order to extinguish it, the lead had been rolled off the bottom of the hip, and some bricks thrown down, all of which were replaced when we arrived at Hecking-  
ham;

ham; but as the men, who had gone up to the corner of the house on the first appearance of the fire, seemed to recollect very well the state in which they had found the lead and tiles at that moment, they were desired to put every thing in the same state to the best of their memory. With this view they turned back the lead at the bottom of the hip on its south side, so that the south-west face of the hip-pole might be seen, and threw down a few tiles, after setting one on edge against the hip-rafter. The lightning then, if such evidence be admitted, had raised up that corner of the lead to the breadth of about six inches at the bottom, and displaced some tiles. An effect of this kind upon the lead, is one of the commonest facts observed in buildings that have been struck by lightning. It so happened, that the piece of lead which we found on the bottom of the hip at Heckingham, had upon it several impressions or pits; concerning which various opinions were entertained, till an experiment, made since our return to town, seems to have put it beyond doubt, that they are nothing but marks of large shot, such as might have been produced by firing, with a large fowling-piece, at a bird sitting on the corner of the house. All the people who assisted in extinguishing the fire agreed, that on the eastern side of the hip, the lead remained, after the stroke, in its usual situation.

On removing entirely the lower part of the lead, no kind of damage was seen on the wood of the hip-pole, except that near the lower end it was slightly scorched in one place, apparently by the flame which had burst forth from below; the spike-nail which had fastened the lead to it appeared perfectly sound, and even the hole made by that nail in the wood was neither burnt nor splintered. This hip-pole was supported, at its proper distance from the hip-rafter, by an iron-strap, or holdfast, which was driven:

driven into the timber making the tie of the angle, through the bevelled end of the hip-rafter, just without the part where the tenon of the latter is received into the mortise of the former (*a*, fig. 8.). Here it was that the fire seemed to have begun, though neither the holdfast itself, nor the hip-pole resting upon it, shewed any signs of the lightning. From the place into which this holdfast was driven (*a*, fig. 8.) to the outer end of the angle-tie (*b*, fig. 8.) there was a considerable loss of substance, occasioning a large hole; but the sides of the hole within were so smooth, and so little charred, shewing plainly the grain of the wood, that it was scarcely possible to suppose the whole had been burnt out; we conjectured, therefore, that a large splinter had been forced off by the lightning at this place, and, in the same moment, the tenon of the hip-rafter set on fire where it enters the mortise. Indeed, unless some opening had been made by forcing out such a piece, it does not appear how the fire could have burnt, for want of air, in a part that is always so closely joined by builders: and yet, in this confined place, the tenon of the hip-rafter was so far consumed, that a ruler could be thrust in, almost to the further extremity of the mortise. From this spot the flame seems to have issued out eastward, between the tie of the angle and the wall-plate (*c*, *c*, fig. 8.) scorching all the timbers it could reach, and setting fire to the laths; but the mischief it had done was very trifling (see fig. 8.).

Just beneath the abovementioned hole at the end of the angle-tie (*b*, fig. 8.), is the extremity of the wall-plate which lies upon the eastern wall of the east flank (*d*, fig. 8.). The end of this wall-plate was rent in a remarkable manner (*e*, *e*, *e*, *e*, fig. 8.), and several of the fissures were continued some way upon the sides (*d* and *f*, fig. 8.). Though the other timbers we have

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mentioned

mentioned are of fir, the wall-plate is of solid oak; and the violence done to its extremity was such, that we could not doubt but it had been occasioned by the lightning.

Under this end of the wall-plate there was a crack in the south face of the corner (*m*, fig. 8.), which went down four courses of bricks, and then terminated abruptly (*m*, fig. 3.). The three external courses of bricks above this crack were new, and projected out much farther than the others, to form the cornice of the wall. Whether the bricks of the old cornice had been damaged or thrown down by the stroke, we could not learn with certainty; but the general report among the people we consulted was, that they had not, and were only taken down to extinguish the fire: this opinion seemed probable from the want of marks on the hip-pole which projected out with the cornice, and the appearance of such strong effects of lightning on the wall-plate which lay within any part of the projection; whence it might be concluded, that the lightning passed within the cornice, and no where through it. Between the bottom of the wall-plate, and the top of the crack where it appeared to begin at the foot of the cornice (*m*, fig. 3. and 8.), lay two inner courses of bricks (*o* and *p*, fig. 8.) covered by the cornice. Some damage had evidently been done to the bricks in this part, though we could not distinctly trace the progress of the lightning through them.

Beneath the east edge of the wall-plate, and separated from it in like manner by two courses of bricks, a similar crack descended from the bottom of the cornice (*l*, fig. 4. and 8.) on the east face of the corner, and went through ten courses of bricks till it reached the top of the wall that supported the stable. Here the three bricks next the house, it is said, were shivered into pieces as small as nuts, but not thrown off (*c*, fig. 10. and 11.). The cracks in the bricks on both faces of the

corner remained, having only been filled up with mortar; but new bricks were put in the place of the three that had been broken on the wall. All the workmen we saw agreed in opinion, that no iron cramps, or other metal, had been used in the brick-work.

Beginning from these three shivered bricks on the top of the wall, three courses of pantiles on the roof of the stable, in the direction downward, were in great measure broken or displaced, except about two feet of the lower end of the courses, near the eaves, where the tiles remained untouched (see *c*, *m*, *g*, *q*, fig. 10. and *c*, *q*, fig. 11.). All these pantiles rested upon laths, which were fastened to the rafters of the roof by iron nails about eleven inches asunder. Within the stable, and almost underneath the spot where the damage to the pantiles ceased, a saddle hung, at the time of the accident, by a nail driven into the wall at that west end of the stable, which was also the eastern wall of the east flank of the house (*n*, fig. 10.). As this saddle, being much torn by the lightning, seems to have been the step by which it passed through the stable, the respective situations of all their parts shall be minutely described.

The stable in its inside is 25 feet long (from *r* to *s*, fig. 9.), 13 feet broad (from *t* to *u*, fig. 9.), 15½ feet high on its south side (from *w* to *x*, fig. 10.), and 7½ feet on the north (from *y* to *z*, fig. 10.). At the west end is a stall for one horse (*s*, fig. 9.). Near the middle of the north wall is a drain (*y*, fig. 9. and 10.), which terminates just without the wall in the garden (*k*, fig. 9. and 10.). Against the west end of the stable, a shelf (*e*, *f*, fig. 10.) was supported by two nails underneath (*f*, fig. 10.). Seven inches and a half below this shelf was a nail, on which the saddle hung by one of its stirrups (*n*, fig. 10.). The breadth of the shelf was near one foot and an half; its length

length (from *e* to *f*, fig. 10.) two feet five inches; and the pantiles seem to have been displaced a little farther down on the roof (*g*, fig. 10.) than the line corresponding perpendicularly with the north end of the shelf (*b*, fig. 10.). Neither the shelf, nor the nails supporting it, which were both near its north end, shewed any signs of injury; whence it may be conjectured, that if the lightning took its course this way, it passed obliquely between the saddle and the roof, so as to miss the edge of the shelf, leaving it to the southward. The upper part of the south side of the stable was boarded off from the rest, to form a hay-chamber, which occupied so large a portion of the roof (from *x* to *m*, fig. 10.), that the boards of the perpendicular partition (at *o*, fig. 10.) came within ten inches of the nail on which the saddle hung. These boards were fastened to the uprights of the partition, all the way down from the roof, by nails about six inches asunder, consequently some of those nails must have been within ten inches of the stirrup-iron as it hung on the nail in the wall (*o* and *n*, fig. 10.). No tokens of the lightning could be discovered on those boards, or the nails fastening them; we could not, therefore, be certain, whether any part of it had passed that way. The nail which supported the saddle was equally free from marks; but one of the stirrup-leathers was much torn and burnt, and a large piece of the leather was stripped off the seat of the saddle, besides other damage done to it in that part. One of the stirrup-irons, likewise, exhibits some appearances of fusion on the arch through which the stirrup-leather passes. This iron, as well as the stirrup-leather, being the only damaged parts of the saddle that remained, we have brought for your inspection.

It must be evident that we derived the knowledge of most of these circumstances relative to the effects of the lightning upon and within the stable from information, the damages having



been repaired before our arrival. As the workmen present, however, agreed pretty well in their testimony, and it was corroborated by every thing that appeared, we desired them to replace all the parts as they were left by the accident, and thence made the descriptions and drawings. We gave directions that a man, accustomed to the stable, should hang up the saddle there in the usual manner, and then ascertained the following measures :

	ft.	in.
From the nails supporting the shelf in the stable (at <i>f</i> , fig. 10.) to the nearest nails of the pantile laths of the roof (about <i>b</i> , fig. 10.)	1	3
From the south end of the shelf ( <i>e</i> , fig. 10.) to the roof over it ( <i>m</i> , fig. 10.)	2	1
N. B. The south end of the shelf was fixed to the partition-boards of the hay-chamber ( <i>e</i> , fig. 10), and the two nails under its north side ( <i>f</i> , fig. 10.) were $5\frac{1}{2}$ inches apart.		
From the nearest of the nails supporting the north end of the shelf ( <i>f</i> , fig. 10.) to the nail on which the saddle hung ( <i>n</i> , fig. 10.)	0	8
Length of the stirrup-iron below the nail ( <i>n</i> , fig. 10.)	0	$3\frac{1}{2}$
Length of the stirrup-leather, from the stirrup-iron to the seat of the saddle	1	9
Breadth of the seat of the saddle	0	8
Distance from the lower side of the seat, as the saddle hung, to the bottom of the lowest stirrup-iron ( <i>p</i> , fig. 10.)	2	0
Distance from the lowest stirrup-iron ( <i>p</i> , fig. 10.) to the floor of the stable (near <i>d</i> , fig. 10. and 9.)	3	6
	As	

As the saddle was thus placed by recollection, the girths reached from it to the ground (*d*, fig. 10. and 9.); but neither these girths, nor any other part of the saddle, except one stirrup-iron, one stirrup-leather, and the seat, were said to have been damaged by the accident.

From the quantity of rain which fell in the thunder-storm, the stable was overflowed with water, which gradually sunk into the drain (at *y*, fig. 9. and 10.). The leather stripped off the seat of the saddle was found in the stable near this drain, whether thrown there originally, or carried by the water, is uncertain. From the point of the floor immediately under the saddle, to the nearest part of the drain, was about 12½ feet; the width of the drain (*y*, fig. 9.) 14 inches; its length through the wall to the edge of the hole or sink into which it discharges itself 18 inches, and the depth of the sink from the bottom of the drain about one foot and an half. As this sink was merely a hole, without any drain leading *from* it, and was bricked at the sides, the water could not pass off by the drain of the stable any faster than it could soak through the loose soil at the bottom of the sink. And it is evident, from this construction, that the earth under the sink will usually be some of the wettest near the building, and be impregnated with salts from the stale of the horses.

Except the marks which have been already described, we could not find on any part of the stable, either within or without, the least vestige of the lightning. We particularly examined the lead *flashing* on the top of the roof (from *c* to *d*, fig. 11.), and the hay-chamber immediately under the three broken bricks and the displaced pantiles, but in vain. There was a hook fixed in the wall, 15 inches below the nail on which the saddle hung, and so exactly underneath, that the stirrup-leather may be

be supposed to have covered it ; but this also appeared to be perfectly untouched. After making every possible inquiry, we could not determine by evidence, whether the stirrup-leather which is so singed and torn was the upper or the lower one at the time of the accident. Much less could we get information of the respective positions of the two stirrup-irons. But, whatever their situation may have been, as so few steps were to be traced, it would seem that the lightning must have jumped over at least one long interval in its passage through the stable.

About seven feet from the stricken corner of the building, and almost two feet from the nearest part of the roof of the stable, is a window (A, fig. 4. 10. and 11.) being the southernmost of the upper range on the east face of the flank. It has thirty small panes of glass, set in lead. We were informed, that about half of these had been broken by the accident, chiefly on the side next the corner ; and that the fissures ran in general horizontally, nearly parallel. Very little, if any, of the glass was forced out. As we could not discover any trace of the lightning directly toward this window, a suspicion arose, whether it might not have been broken rather by the general concussion than by any immediate stroke.

Having examined all the marks that appeared between the bottom of the stricken hip and the ground, our next inquiries were directed to the top of the hip (f, fig. 2. 3. 4. and 11.). Here the upper plate of lead (e, fig. 4.) which served as a capping to the junction of the hip with the ridge of the roof, being taken off, we found, on its under surface, three distinct marks of fusion ; and on the upper surface of the sheet of lead which it covered three corresponding marks, so exactly similar, that the two surfaces of lead seem to have touched one another in a melted state. These fused spots are just in the bend of the lead,

lead, answering to the obtuse angle formed between the hip and the roof (*f*, fig. 4.). We obtained leave to bring away both the pieces of lead, and now present them for your inspection. The workmen who examined the timber underneath reported, that it was not damaged; nor were any other signs of the lightning perceived in the whole length of this strip of lead from the top to the bottom of the hip. In the pieces of lead which exhibit the melted spots on one surface, the other surface is perfectly clear of all marks, though the latter was, in the uppermost plate of lead, that which had lain exposed to the clouds. Neither of them is melted to any depth into the substance of the metal.

As both extremities of the hip, therefore, were, in some degree at least, affected by the lightning, we proceeded to ascertain their distances from the nearest conductor, which was that affixed to the chimney E (fig. 2. and 4.). Having determined the necessary measures, and calculated the hypotenuse, the distance from the point of the conductor to the beginning of the lead on the top of the hip (*e*, fig. 4.) came out 42 feet and a quarter; thence to the bend where we found the marks of fusion (from *e* to *f*, fig. 4.) was five or six inches more; and as the hip measured about 27 feet in length, the distance from the conductor to the bottom of the hip (*g*, fig. 4.) may be called 69 feet. From the top or bottom of the hip, to the nearest part of the conductor as it ran downward, the distances were not a foot less than these measures. We then took down the uppermost rod of the conductor, and carefully examined it, especially at the point, and at the hook and screws by which it had been joined to the second rod; but could no where discover the least mark of fusion or other injury. At the bottom of this conductor, however, where, having joined that from the chimney D,

D. it terminated in the drain (see the general plan, fig. 1. and 2, fig. 5.), a small bright spot appeared on one of the angles. As some suspicions were entertained, whether this mark might not have been occasioned by the lightning, we cut off the end of the rod, and have brought it hither for public examination.

Where this conductor entered its channel at the corner of the court (see T, fig. 1. and 2, fig. 2. and 5.), the ground is raised so much above the grate of the sink (W, fig. 1. and 5.) that, though the court was overflowed, it is not probable, the water could have risen high enough to run into the channel (at 2, fig. 5.), and so communicate with the conductor before it reached the drain.

Close to the chimney E, a dinner-bell hung in a common frame (q, fig. 2. and 4.). Three different persons went up to examine this bell; but could not discover upon it any where the least vestige of the lightning.

Such are the facts we were able to collect from an assiduous examination of the Poor-house at Heckingham, and of those witnesses in the neighbourhood who knew any thing of the accident. We have stated the appearances as they presented themselves to us, with all the minuteness that could be preserved without too much crowding the narrative, and independently of any opinions. Whether the earth or the clouds were positive at the time; whether the top or bottom of the hip was first affected by the stroke; whether all the lightning took its course through the hip, or part went that way, and part through the conductor; and how far the conductors were properly constructed, or adequately terminated; are questions which will naturally suggest themselves to your consideration.

It

It may be proper, however, to add the two following pieces of information.

One of the cripples in the House of Industry, a middle-aged woman, assured us, that at the time of the accident, as she was looking from the door of the hall (which is in the center of the front facing the south), she saw three balls of fire dart down; that one fell exactly opposite her; a second seemed to strike the corner of the house; and the other descended in the direction of a door in the eastern flank, which was not far out of the perpendicular line of the chimney E (see the general plan, fig. 1.). If any credit could be given to the testimony of such a person in a matter like this, it would incline us to believe, that the explosion was made in three streams, of which one passed through the conductor of the chimney E, and another through the damaged corner of the house; whilst the third fell on the ground, or, as the woman described it, on the great gate of the fore-court near the lodges (see the general plan, fig. 1.). We examined the gate and lodges, with the adjacent parts, but could nowhere discover any marks of injury; nor could we learn that any place in the neighbourhood had been struck, or that any person, except this woman, pretended to have seen the course of the lightning.

In our return to town, through Norwich, we saw an ingenious gentleman of that city, who says, that he found the clouds negative there on the day of the accident at Heckingham. The two places are distant about eleven miles by the road.

It would be unpardonable to conclude this Report, without expressing our obligations to the Directors and Guardians of the House of Industry at large, and to the neighbouring

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D d d

Gentlemen

Gentlemen in particular, for the liberal manner in which they seconded our endeavours to execute the commission with which you had charged us. By their kind assistance proper workmen were provided; and every accommodation afforded us, that could contribute to the investigation of this remarkable accident.

We have the honour to be,

GENTLEMEN,

Your most obedient humble servants,

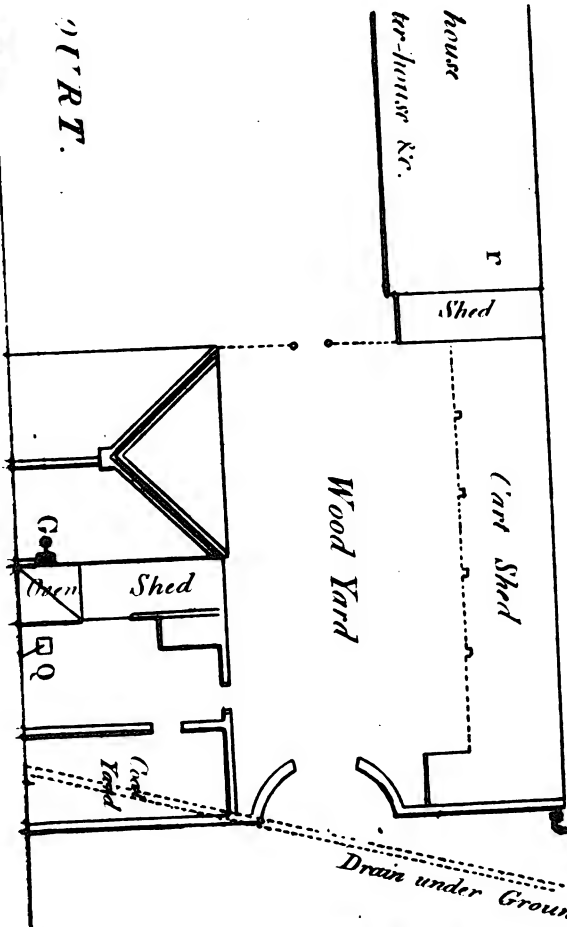
C. BLAGDEN.

EDW. NAIRNE.

London, Feb. 7, 1782.

# PLAN.

D R T H.



W R T.

Reservoir











1724

112



**Fig.VIII.**

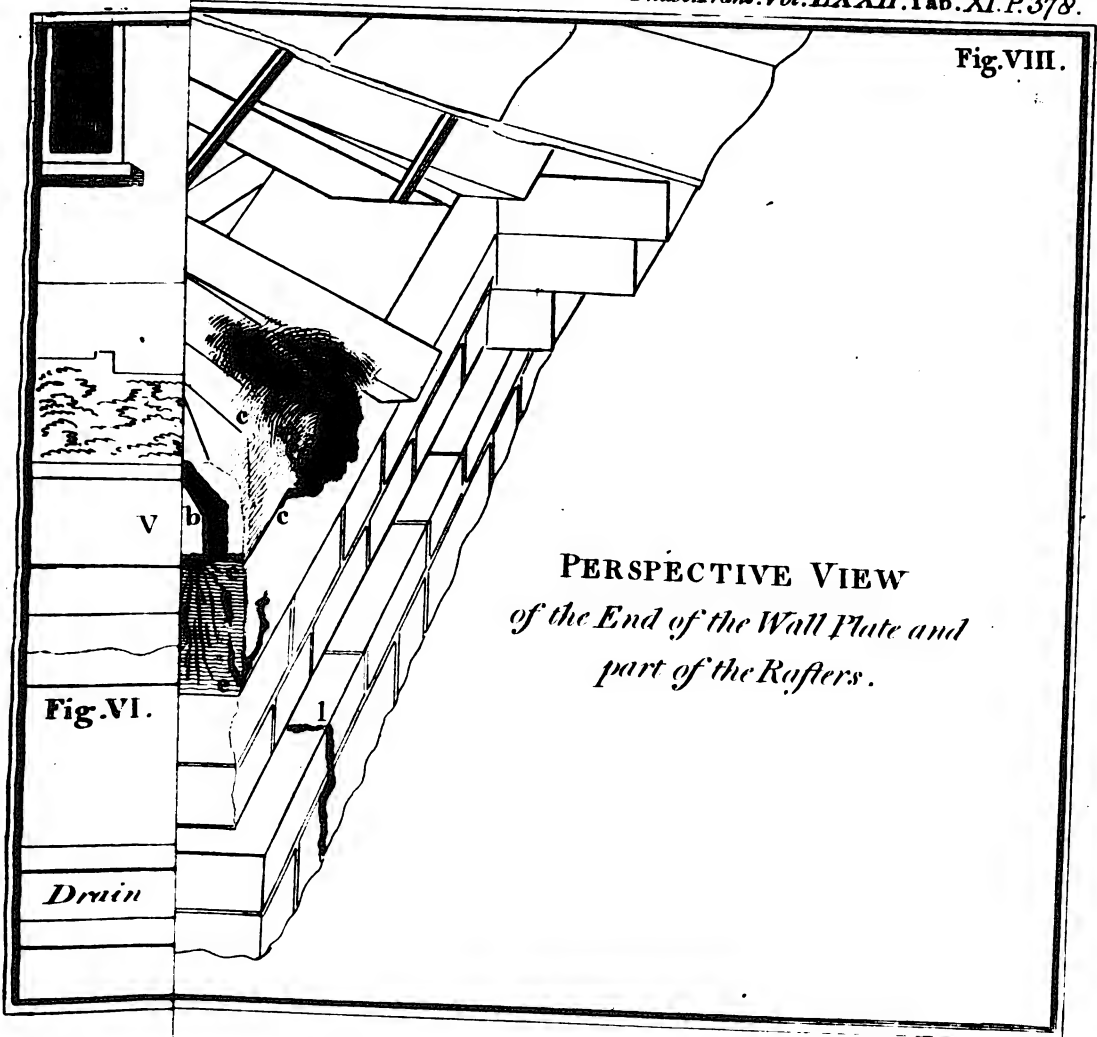
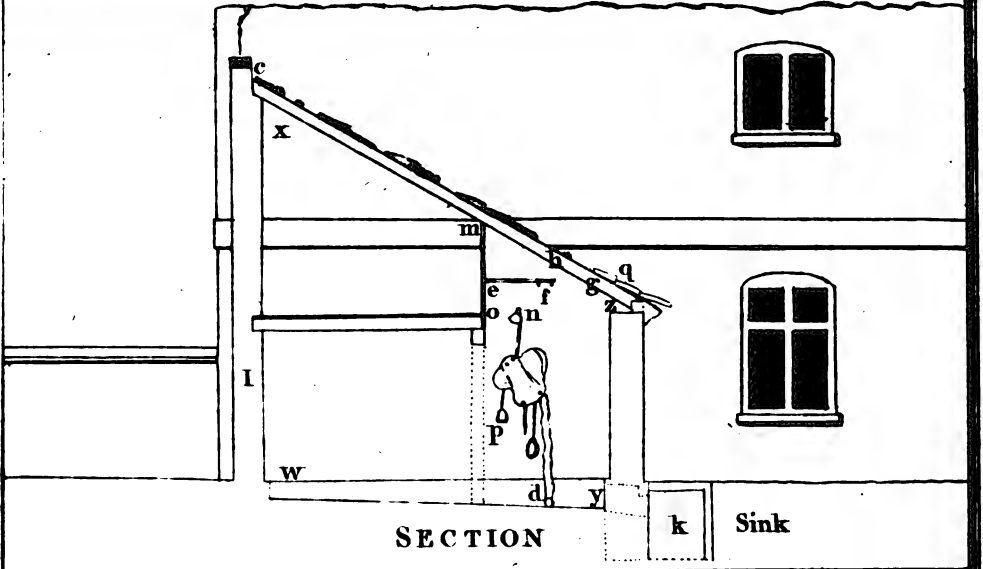




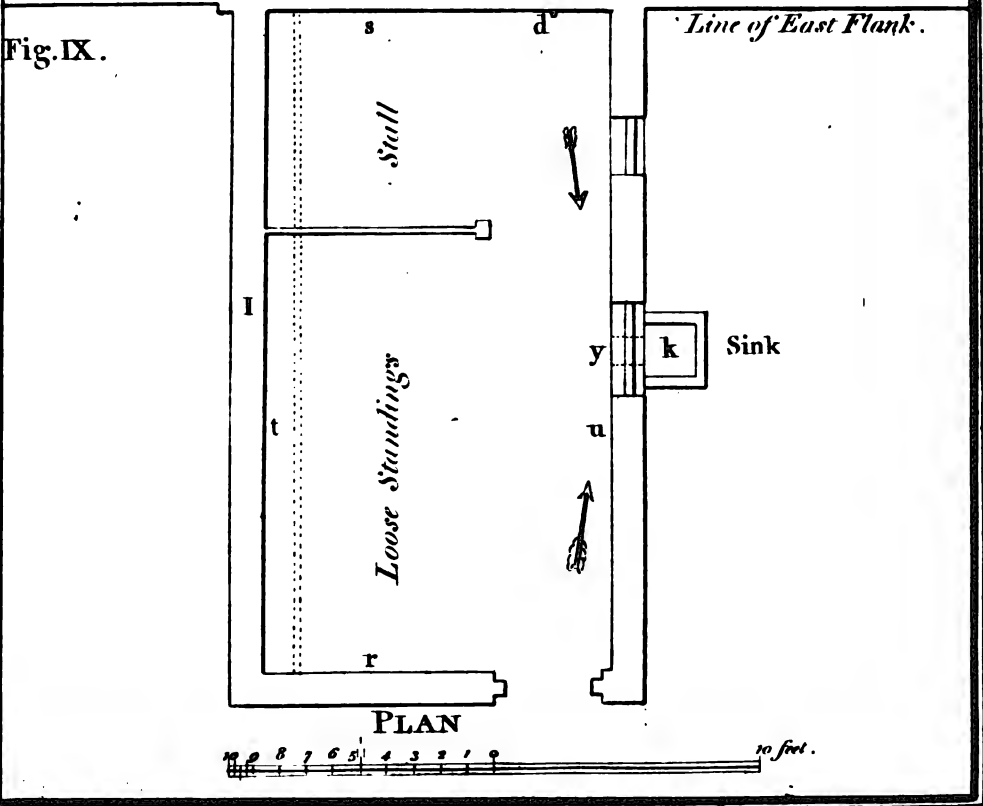
Fig. X.

PLAN and SECTION of the Stable Lean-to.



Fence Wall

Fig. IX.









1871

1872

1873

1874

1875

1876

1877

1878

XXIII. *Account of the Organ of Hearing in Fish.*By John Hunter, *Esq.* F. R. S.

Read Nov. 14, 1782.

NATURAL history has ever been considered as worthy the attention of the curious philosopher, and therefore has in all ages kept pace with the other branches of knowledge; and as both arts and sciences have, of late years, been cultivated to a degree, perhaps, beyond what was ever known before, we find also, that natural history has not been neglected; all Europe appears to be awake to it. In this island it has been pursued with more philosophic ardour, than what was ever known in any country. It has become the study of men of independent fortunes, who not only spend their fortunes in the cultivation of this science, but have risked their health and lives in pursuit of it, searching unknown regions to improve mankind, settling correspondences every where, so as to bring in its materials into this country, in order to make it the school of natural history. It is no wonder, then, that a spirit of inquiry is diffused through almost all ranks of men; and that though many cannot pursue it themselves, yet they are eager to know what is already known, chusing at least to benefit by the industry of others.

These reflections have induced me to trouble this learned Society with a short account of the Organ of Hearing in

D d d 2

Fish,

Fish, it being still a subject of great dispute, whether fish hear or not.

Some time between the years 1750 and 1760, I observed the organ of hearing in fish; and from that time to this, I only considered it as a link in the chain of the varieties in this sense in different animals, in which there is a regular progression, *viz.* from the most perfect animals down to the most imperfect possessed of this organ\*.

As I do not intend to give, in this paper, a full account of this organ in any one fish, or of the varieties in different fish, but only of the organ in general; those who may chuse to pursue this part only of the animal œconomy may think it deficient in the descriptive parts. If it was a difficult task to expose this organ in fish, I should perhaps be led to be more full in my description of it, but there is nothing more easy than the exposure of this organ in this animal in general.

As this paper is to be confined to this order of animals, I may be allowed just to observe here, that the class called sepia has this organ also, but somewhat differently constructed from what it is in the fish.

The organs of hearing in this latter order of animals are placed on the sides of the skull, or that cavity which contains the brain; but the skull itself makes no part of the organ, as it does in the quadruped and the bird. In some fish this organ is wholly surrounded by the parts composing this cavity, which in many is cartilaginous, the skeleton of these fish being

\* Preparations to illustrate these facts have been ever since shewn in my collection to the curious both of this country and foreigners: when in shewing whatever was new, or supposed to be new, the ears of fish were always considered by me as one important article.

like

like those of the ray kind; in others also, as in cod, salmon, &c. whose skeleton is bone, yet this part is cartilaginous.

In some fish this organ is in part within the cavity of the skull, or that cavity which also contains the brain, as in the salmon, cod, &c. the cavity of the skull projecting laterally, and forming a cavity there.

The organ of hearing in fish appears to grow in size with the animal, for its size is nearly in the same proportion with the size of the animal, which is not the case with the quadruped, &c. the organs being in them nearly as large in the growing foetus as in the adult.

It is much more simple in fish than in all those orders of animals who may be reckoned superior, such as quadrupeds, birds, and amphibious animals, but there is a regular gradation from the first to fish.

It varies in different orders of fish; but in all it consists of three curved tubes, all of which unite with one another; this union forms in some only a canal, as in the cod, salmon, ling, &c.; and in others, a pretty large cavity as in the ray kind. In the jack there is an oblong bag, or blind process, which is an addition to those canals, and which communicates with them at their union. In the cod, &c. this union of the three tubes stands upon an oval cavity, and in the jack there are two of those cavities; these additional cavities in these fish appear to answer the same purpose with the cavity in the ray or cartilaginous fish, which is the union of the three canals.

The whole is composed of a kind of cartilaginous substance, very hard or firm in some parts, and which in some fish is cruusted over with a thin bony lamella, so as not to allow them to collapse; for as the skull does not form any part of

those.

those canals or cavities they must be composed of such substance as is capable of keeping its form.

Each tube describes more than a semi-circle. This resembles in some respect what we find in most other animals, but differs in the parts being distinct from the skull \*.

Two of the semi-circular canals are similar to one another, may be called a pair, and are placed perpendicularly; the third is not so long; in some it is placed horizontally, uniting as it were the other two at their ends or terminations. In the skait it is something different, being only united to one of the perpendiculars.

The two perpendiculars unite at one part in one canal, by one arm of each uniting, while the other two arms or horns have no connection with each other, and the arms of the horizontal unite with the other two arms of the perpendicular near the entrance into the common canal or cavity.

Near the union of those canals into the common, they are swelled out into round bags, becoming there much larger.

In the ray kind they all terminate in one cavity, as has been observed; and in the cod they terminate in one canal, which in these fish is placed upon the additional cavity or cavities. In this cavity or cavities there is a bone or bones. In some there are two bones; as the jack has two cavities, we find in one of those cavities two bones, and in the other only one; in the ray there is only a chalky substance †.

At this union of the two perpendiculars in some fish enters the external communication, or what may be called the external meatus. This is the case with all the ray kind, the external orifice

\* The turtle and the crocodile have a structure somewhat similar to this; and the intention is the same, for their skulls make no part of the organ.

† This chalky substance is also found in the ears of amphibious animals.

of which is small, and placed on the upper flat surface of the head; but it is not every genus or species of fish that has the external opening.

The nerves of the ear pass outwards from the brain, and appear to terminate at once on the external surface of the swelling of the semi-circular tubes above described. They do not appear to pass through those tubes so as to get on the inside, as is supposed to be the case in quadrupeds; I should therefore very much suspect, that the lining of those tubes in the quadruped is not nerve, but a kind of internal periosteum.

As it is evident that fish possess the organ of hearing, it becomes unnecessary to make or relate any experiment made with live fish which only tends to prove this fact; but I will mention one experiment, to shew that sounds affect them much, and is one of their guards, as it is in other animals. In the year 1762, when I was in Portugal, I observed in a nobleman's garden, near Lisbon, a small fish-pond, full of different kinds of fish. Its bottom was level with the ground, and was made by forming a bank all round. There was a shrubbery close to it. Whilst I was laying on the bank, observing the fish swimming about, I desired a gentleman, who was with me, to take a loaded gun, and go behind the shrubs and fire it. The reason for going behind the shrubs was, that there might not be the least reflection of light. The instant the report was made, the fish appeared to be all of one mind, for they vanished instantaneously into the mud at the bottom, raising as it were a cloud of mud. In about five minutes after they began to appear, till the whole came forth again.





XXIV. *Account of a new Electrometer.* By Mr. Abraham Brook; communicated by Sir Joseph Banks, Bart. P. R. S.

Read May 30, 1782.

AAAAN, fig. 1. represents the electrometer in full size and proportion, standing on a table, or the like. The foot B is a square piece of board,  $9\frac{1}{4}$  inches each way, resting on three pins C, C, c, seen at the under side of the foot. C, C, with the broad heads, are screws to set the instrument upright withal. D is a solid piece of glass, which supports and insulates the instrument from the place on which it stands. The arms G<sub>1</sub> and g, with the ball F, turn round on the wire H (which is solid brass, as may be also the arm g), and, when in use, are put near at a right angle with G<sub>2</sub> and H, being turned to the off side so as to be as much as possible out of each other's atmospheres or the atmosphere of a jar, battery, prime conductor, &c. The arms G<sub>1</sub> and G<sub>2</sub> are hollow tubes of copper, not so heavy as wires. The balls I<sub>1</sub>, I<sub>2</sub>, are made of copper, and hollow, so as to be as light as possible. K represents a kind of face or dial plate to the instrument with its index, which is carried once round by the motion of the arm G<sub>2</sub> with its ball I<sub>2</sub> moving through a quarter, or 90 degrees, of a circle; this motion is given to it by the repulsive power of the charge, &c. of electricity between the two balls I<sub>2</sub> and L. The ends of the index from its center are of different lengths. The longest end reaches to a graduated circle, divided into 90 equal parts, answering

answering to the  $90^\circ$ , which the arm  $G_2$  moves through. The shortest end reaches to a smaller circle, divided into 60 equal parts, answering to 60 grs. weight, or 60 divisions, on the arm  $G_1$ , with its sliding weight  $m$ , each of which is equal to one grain, and the whole face is covered with a watch glass, to prevent the electricity from flying off at the points.

The top of the glass-supporter, or insulator  $D$ , is cemented into a brass cap  $M$ . This cap enters the ball  $L$  at bottom, and screws into the upper part of the ball  $L$  at  $a$ . The top part of this cap  $M$  is tapered off to a cone about an inch and a half long or high. The lower end of the wire  $H$  has a hole made conically into it, so as to receive the upper part, or conical end, of the cap  $M$ , which permits all the upper part of the electrometer to turn round any way that may be necessary. The kind of ferrel  $O$ , with its base, is perforated for the lower end of the wire  $H$  to go through. The bent arm  $b$ , which supports the cup  $N$ , is screwed into the base of the ferrel  $O$ , and turns freely round upon the wire  $H$ . The cup  $N$  is to receive the ball  $P$  of the arm, fig. 9. This arm shortens or lengthens, as may be wanted, by a wire sliding into a tube. The end of the wire is slit, forming a spring in the tube to be steady. In this arm, fig. 9. is a kind of rule joint at  $d$ , that the arm may give way easily if wanted. The semi-circular end of the arm is a spring, and slips on to a ball from the prime-conductor, or the conductor itself (if they fit), jar, or battery. The ends of it are flat and broad, as represented in the drawing in miniature, of the electrometer at fig. 2. in the other drawing.

Fig. 2. to 11. shews the internal structure of the electrometer.

Fig. 12. shews the part at  $x$  that screws into the ball  $F$ , to support the arm  $g$  with its ball  $r$ . This piece, which is made

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hollow

hollow on the side next the wire H, so as to fit, and is screwed in, so as to press against H, serves as a spring to keep the ball F steady, which slides up and down, as well as turns round, on H.

In order to make the divisions of G1, fig. 1. exactly a grain each, first slide the weight *m* towards the ball F, fig. 1. till it is an exact counter-balance to the weight in F. At one end of the weight *m* let the divisions begin; then suspend any tolerable pair of scales, so that the bottom of one of them may rest on the top of the ball *r*; then lay the ball I1 into the scale, and slide the weight *m* near to I1, and put as many grains into the other scale as will just raise the ball I1 in the scale; then mark the arm G1 at the same end of the weight *m*, and divide the space between the two marks into as many parts as there are grains in the scale, which may be divided and sub-divided into halves and quarters.

The arm G2 being repelled shews when the charge is increasing, &c.; and I1 tells what such a repulsive power is between two balls of the size of these in grains, according to the number the weight *m* rests at when lifted up by the repulsive power of a charge. The longest end of the index K shews how many degrees of a circle G2 is repelled; and, by many trials, according to the number of grains, the arm G1 shews, when it is lifted up, and the weight *m* put at different places, such respective numbers of grains may be marked on the least graduated circle on the dial plate where the shortest end of the index points; so that when all the grains are thus marked on the dial plate, thus ascertained by the arm G1, all these parts of the instrument, that is, the ball F with the arms G1 and *g* may be taken off, and the instrument is then graduated to be used without them; but I do not know how the grains can be so  
exactly

exactly marked and ascertained as by these parts being on the instrument: nor do I mean to confine the number of grains or divisions on  $G_1$ ; but, I think, my experience seems to tell me, that no glass to be *charged*, as we call it, with electricity, will bear a greater charge than that whose repulsive force, between two balls of this size, equals 60 grains weight, before it will be perforated or struck through. Nay, I have not found many instances where it would stand 50 grains; and, I think, it is very hazardous to go more than 45 grains.

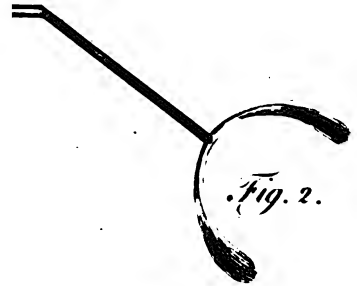
Thus, by knowing the quantity of coated surface, and the diameter of the balls, as  $I_1$  and  $r$ , I would say, so much or so much coated surface charged to so many or so many grains: repulsion between two balls of such or such a size would melt a wire of this or that size, or do such a thing, kill such an animal, &c.; and if balls, wires, or arms of this size, are found too small, larger may be made on the same plan.

In respect to the advantages of this electrometer above *those now in use*, I do not, perhaps, know them all; and lest my partiality may prejudice me in behalf of my own contrivance, would rather leave them to the judgement of others; my opinion however is, that all that I have seen or heard of are such as speak no intelligible language, and that this speaks so as to be understood universally; for, unless the repulsive power of the charge of different glasses be very different, this electrometer, or any other electrometer, made after this manner, must, I should think, speak very nearly the same language, it being known how much coated surface there is, and the size of the balls; but if the size of the balls be not the same, the language the instrument speaks will be very different. Although other Electrometers shewed a

greater or lesser charge or power, by an arm being repelled to a greater or lesser distance, or by striking differently at different distances, yet the power of the charge was not in any manner ascertained; we could say, that the arm or index was repelled to such or such a number of degrees of a circle, or that it struck to such or such a distance; but the repulsive power of a charge to repel the index so much, or so many degrees of a circle, or the strength of the charge to strike to such a distance was not (that I know of) in any manner intelligibly ascertained. This shews it by the weight that the repulsive power has to lift up in grains, &c.; which weight is to be proved by any tolerable pair of scales and weights; and I do not know any other method that has been yet tried to shew the different strength of charges so good as that of repulsion.

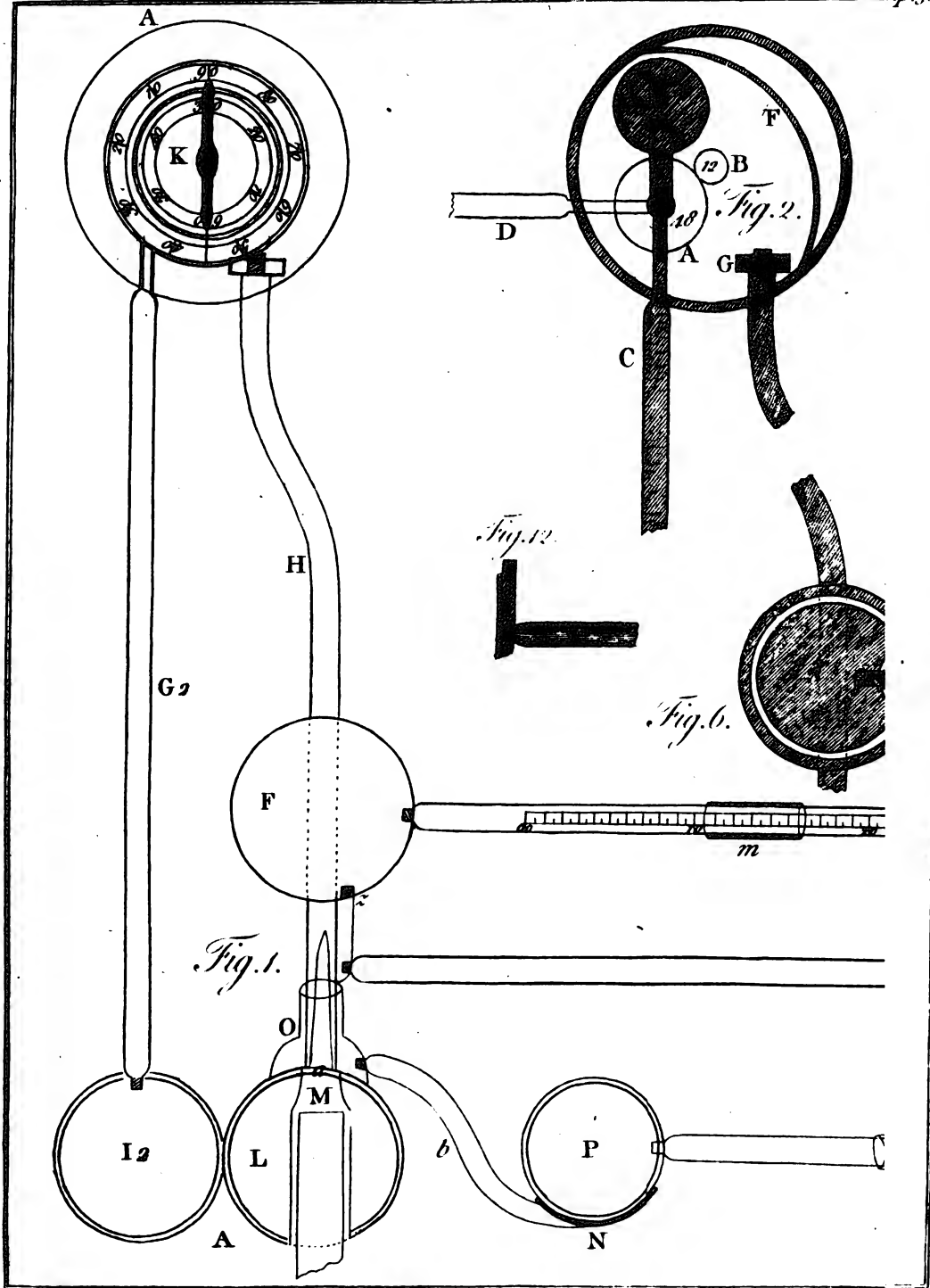
All the necessary parts of the instrument being made of metal and glass that is pretty stout, I think, the electricity is less liable to escape than by wood, &c. I have tried reeds on account of their being light, and covered them with tin-foil, or gilded them to make them good conductors; but so frequently found inconveniencies from them by points rising up, the celerity of moving, and the different weight of them at different times owing to moisture, change of the weather, and the like, that I have laid them all aside, and find my present instrument as free from these inconveniencies as I could expect; nor is it liable to be out of order, if proper care is taken of it.





*Electrometer in miniature. Fig. 2.  
communication from a Battery, &c.  
being slipt on to a Ball of the  
Battery &c. and the Ball  $\Delta$  being laid into the Cup  $\Delta$  Fig. 1.*

1. The first part of the book is a general introduction to the subject of the history of the English language, and is divided into two chapters, the first of which deals with the history of the English language from the beginning of the world to the present time, and the second with the history of the English language from the beginning of the world to the present time.







XXV. *A new Method of investigating the Sums of infinite Series.*

*By the Rev. S. VINCE, A. M. of CAMBRIDGE, in a Letter to Henry Maty, A. M. Secretary.*

Read June 6, 1782.

SIR,

HAVING lately discovered some very easy methods of investigating the sums of certain infinite series, I have taken the liberty of requesting the favour of you to present them to the Royal Society. I have divided the subject into three parts: the first contains a new and general method of finding the sum of those series which DE MOIVRE has found in one or two particular cases; but whose method, although it be in appearance general, will, upon trial, be found to be absolutely impracticable. The second contains the summation of certain series, the last differences of whose numerators become equal to nothing. The third contains observations on a *correction* which is necessary in investigating the sums of certain series by collecting two terms into one, with its application to a variety of cases.

I am, &c.

Cambridge,  
May 3, 1782.

PART

## P A R T I.

## L E M. I.

Let  $r$  be any whole number, and then the fluent of  $\frac{x^r}{1+x^r}$  can always be exhibited by circular arcs and logarithms; but when  $x=1$ , the fluent of the same fluxion will be expressed by the infinite series  $1 - \frac{1}{r+1} + \frac{1}{2r+1} - \frac{1}{3r+1} + \&c.$  the sum of this series therefore can always be found by circular arcs and logarithms.

## L E M. II.

To find the sum of the infinite series  $\frac{a}{1 \cdot r+1} - \frac{a+b}{r+1 \cdot 2r+1} + \frac{a+2b}{2r+1 \cdot 3r+1} - \&c.$

Assume  $1 - \frac{1}{r+1} + \frac{1}{2r+1} - \frac{1}{3r+1} + \&c. = S$ ; therefore,

$$(A) \quad 1 - \frac{1}{1 \cdot r+1} + \frac{r+1}{r+1 \cdot 2r+1} - \frac{2r+1}{2r+1 \cdot 3r+1} + \&c. \dots = S.$$

In the first series, add together the 1st and 2d, the 2d and 3d, &c. &c. terms, and the resulting series will evidently be equal to twice that series *minus* the first term; therefore,

$$(B), \quad \frac{r}{1 \cdot r+1} - \frac{r}{r+1 \cdot 2r+1} + \frac{r}{2r+1 \cdot 3r+1} - \&c. \dots = 2S - 1.$$

$$\text{Now } \left( \frac{A}{r} \right) \frac{1}{r} - \frac{\frac{1}{r}}{1 \cdot r+1} + \frac{1 + \frac{1}{r}}{r+1 \cdot 2r+1} - \frac{2 + \frac{1}{r}}{2r+1 \cdot 3r+1} + \&c. \dots = \frac{S}{r},$$

$$\left. \begin{aligned} \text{or } & \frac{1}{r+1 \cdot 2r+1} - \frac{2}{2r+1 \cdot 3r+1} + \&c. \dots \\ & + \frac{1}{r} - \frac{\frac{1}{r}}{1 \cdot r+1} + \frac{\frac{1}{r}}{r+1 \cdot 2r+1} - \&c. \dots \end{aligned} \right\} = \frac{S}{r};$$

Now

Now the sum of the lower series, omitting the first term, is equal to  $-B$  divided by  $r^2$ , or  $= -\frac{2S-1}{r^2}$ ; hence, by transposition, and, multiplying both sides by  $b$ , we shall have,

$$\frac{b}{r+1 \cdot 2r+1} - \frac{2b}{2r+1 \cdot 3r+1} + \&c. \dots = \frac{bS}{r} + \frac{2bS-r+1 \cdot b}{r^2}; \text{ also by multiplying } B \text{ by } \frac{a}{r} \text{ we have}$$

$$\frac{a}{1 \cdot r+1} - \frac{a}{r+1 \cdot 2r+1} + \frac{a}{2r+1 \cdot 3r+1} - \&c. \dots = \frac{2aS-a}{r}; \text{ subtract the last equation but one from the last, and we shall have}$$

$$\frac{a}{1 \cdot r+1} - \frac{a+b}{r+1 \cdot 2r+1} + \frac{a+2b}{2r+1 \cdot 3r+1} - \&c. \dots = \frac{2ra-r+2 \cdot b \times S - ra+r+1 \cdot b}{r^2}.$$

Cor. 1. Hence it appears, that the sum of this series can never be exhibited in finite terms, except  $a:b$  as  $r+2:2r$ , in which case the sum is equal to  $\frac{a}{r+2}$ .

$$\text{Hence, if } a=3, b=2, \text{ then } r=1; \therefore \frac{3}{1 \cdot 2} - \frac{5}{2 \cdot 3} + \frac{7}{3 \cdot 4} - \&c. \dots = 1;$$

$$\text{if } a=1, b=4, \text{ then } r=\frac{4}{3}; \therefore \frac{5}{3 \cdot 7} - \frac{9}{7 \cdot 11} + \frac{13}{11 \cdot 15} - \frac{17}{15 \cdot 19} + \&c. \dots = \frac{1}{6};$$

$$\text{if } a=4, b=3, \text{ then } r=\frac{6}{5}; \therefore \frac{4}{5 \cdot 11} - \frac{7}{11 \cdot 17} + \frac{10}{17 \cdot 23} - \frac{13}{23 \cdot 29} + \&c. \dots = \frac{1}{20};$$

Cor 2. Put  $a=c-b$ , and we shall have, after transposition,

$$\frac{c}{r+1 \cdot 2r+1} - \frac{c+b}{2r+1 \cdot 3r+1} + \&c. \dots = \frac{3r+2 \cdot b - 2rc \times S - 2r+1 \cdot b + rc}{r^2} + \frac{c-b}{r+1}.$$

## P R O P. I.

To find the sum of the infinite series  $\frac{m}{1 \cdot r+1 \cdot 2r+1} +$

$$\frac{m+n}{2r+1 \cdot 3r+1 \cdot 4r+1} - \frac{m+2n}{4r+1 \cdot 5r+1 \cdot 6r+1} + \&c.$$

Every

Every series of this kind may be resolved into the following series  $\frac{a}{1 \cdot r+1} - \frac{a+b}{r+1 \cdot 2r+1} + \frac{a+2b}{2r+1 \cdot 3r+1} - \frac{a+3b}{3r+1 \cdot 4r+1} + \&c.$  for if we reduce two terms of this series into one, it will become

$$\frac{2ar-b}{1 \cdot r+1 \cdot 2r+1} + \frac{2ra+2r-1 \cdot b}{2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{2ra+4r-1 \cdot b}{4r+1 \cdot 5r+1 \cdot 6r+1} + \&c.$$

where the denominators being the same as in the given series, and the numerators also in arithmetic progression, we have only to take  $a$  and  $b$  such quantities that the respective numerators may be also equal; assume, therefore,  $2ra-b=m$ ,  $2ra+2r-1 \cdot b=m+n$ ; therefore,  $b=\frac{n}{2r}$ ,  $a=\frac{2rm+n}{4r^2}$ , which substituted for  $a$  and  $b$  in LEM. 2. gives

$$\begin{aligned} & \frac{m}{1 \cdot r+1 \cdot 2r+1} + \frac{m+n}{2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{m+2n}{4r+1 \cdot 5r+1 \cdot 6r+1} + \&c. \dots \\ &= \frac{2rm-r+1 \cdot n}{2r^2} \times S + \frac{2r+1 \cdot n-2rm}{4r^2}. \end{aligned}$$

Let  $r=1$ , and we have

$$\frac{m}{1 \cdot 2 \cdot 3} + \frac{m+n}{3 \cdot 4 \cdot 5} + \frac{m+2n}{5 \cdot 6 \cdot 7} + \&c. \dots = m - n \cdot S + \frac{3n-2m}{4}.$$

$$\text{If } m=1, n=3, \frac{1}{1 \cdot 2 \cdot 3} + \frac{4}{3 \cdot 4 \cdot 5} + \frac{7}{5 \cdot 6 \cdot 7} + \&c. \dots = \frac{7}{4} - 2S;$$

$$m=1, n=0, \frac{1}{1 \cdot 2 \cdot 3} + \frac{1}{3 \cdot 4 \cdot 5} + \frac{1}{5 \cdot 6 \cdot 7} + \&c. \dots = S - \frac{1}{2}.$$

Let  $r=2$ , and the series becomes

$$\frac{m}{1 \cdot 3 \cdot 5} + \frac{m+n}{5 \cdot 7 \cdot 9} + \frac{m+2n}{9 \cdot 11 \cdot 13} + \&c. \dots = \frac{4m-3n}{16} \times S + \frac{5n-4m}{32}.$$

$$\text{If } m=1, n=1, \frac{1}{1 \cdot 3 \cdot 5} + \frac{2}{5 \cdot 7 \cdot 9} + \frac{3}{9 \cdot 11 \cdot 13} + \&c. \dots = \frac{S}{16} + \frac{1}{32};$$

$$m=1, n=0, \frac{1}{1 \cdot 3 \cdot 5} + \frac{1}{5 \cdot 7 \cdot 9} + \frac{1}{9 \cdot 11 \cdot 13} + \&c. \dots = \frac{S}{4} - \frac{1}{8}.$$

Let  $r=5$ , and we shall have

$$\frac{m}{1 \cdot 6 \cdot 11} + \frac{m+n}{11 \cdot 16 \cdot 21} + \frac{m+2n}{21 \cdot 26 \cdot 31} + \&c. \dots = \frac{5m-3n}{125} \times S + \frac{11n-10m}{500}.$$

$$\text{If } m=1, n=1, \frac{1}{1 \cdot 6 \cdot 11} + \frac{2}{11 \cdot 16 \cdot 21} + \frac{3}{21 \cdot 26 \cdot 31} + \&c. \dots = \frac{2}{125} \times S + \frac{1}{500};$$

$$m=1, n=0, \frac{1}{1 \cdot 6 \cdot 11} + \frac{1}{11 \cdot 16 \cdot 21} + \frac{1}{21 \cdot 26 \cdot 31} + \&c. \dots = \frac{8}{25} - \frac{1}{50}.$$

Cor. If  $2r : r+1 :: n : m$ , the sum of the series can be accurately found, and will be equal to  $\frac{m}{2r \cdot r+1}$ . Let therefore

$m=r+1$ , and then  $n=2r$ , consequently

$$\frac{1}{1 \cdot 2r+1} + \frac{1}{2r+1 \cdot 4r+1} + \frac{1}{4r+1 \cdot 6r+1} + \&c. \dots = \frac{1}{2r};$$

which is also known from other principles.

## P R O P. II.

To find the sum of the infinite series  $\frac{m}{r+1 \cdot 2r+1 \cdot 3r+1} +$

$$\frac{m+n}{3r+1 \cdot 4r+1 \cdot 5r+1} + \frac{m+2n}{5r+1 \cdot 6r+1 \cdot 7r+1} + \&c.$$

This series resolves itself into

$$\frac{c}{r+1 \cdot 2r+1} - \frac{c+b}{2r+1 \cdot 3r+1} + \frac{c+2b}{3r+1 \cdot 4r+1} - \&c.;$$

for by reduction, as before, it becomes

$$\frac{2cr-r+1 \cdot b}{r+1 \cdot 2r+1 \cdot 3r+1} + \frac{2cr+r-1 \cdot b}{3r+1 \cdot 4r+1 \cdot 5r+1} + \frac{2cr+3r-1 \cdot b}{5r+1 \cdot 6r+1 \cdot 7r+1} + \&c.$$

where the denominators are the same as in the given series, and the numerators in arithmetic progression; assume therefore

$$2cr-r+1 \cdot b = m, \quad 2cr+r-1 \cdot b = m+n, \quad \text{hence } b = \frac{n}{2r},$$

$$s = \frac{2rm+r+1 \cdot n}{4r^2}, \quad \text{which, substituted in cor. 2. LEM. 2. give}$$

$$\begin{aligned} & \frac{m}{r+1 \cdot 2r+1 \cdot 3r+1} + \frac{m+n}{3r+1 \cdot 4r+1 \cdot 5r+1} + \frac{m+2n}{5r+1 \cdot 6r+1 \cdot 7r+1} + \&c. \dots \\ & = \frac{2r+1 \cdot n - 2rm}{2r^3} \times S + \frac{2rm-3r+1 \cdot n}{4r^3} + \frac{2rm-r-1 \cdot n}{4r^3 \cdot r+1}. \end{aligned}$$

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Cor.

Cor. 1. In prop. 1. substitute  $a$  for  $m$ , and  $2b$  for  $n$ , and we have

$$\frac{a}{1 \cdot r + 1} + \frac{a+2b}{2 \cdot r + 1} + \frac{a+4b}{3 \cdot r + 1} + \frac{a+6b}{4 \cdot r + 1} + \&c. \dots =$$

$$\frac{ra-r+1 \cdot b}{r^3} \times S + \frac{2r+1 \cdot b-ra}{2r^3}$$

Also in this prop. substitute  $a+b$  for  $m$ , and  $2b$  for  $n$ , and we have,

$$\frac{a+b}{r+1 \cdot 2r+1 \cdot 3r+1} + \frac{a+3b}{3r+1 \cdot 4r+1 \cdot 5r+1} + \&c. \dots =$$

$$\frac{r+1 \cdot b-ra}{r^3} \times S + \frac{ra-2r+1 \cdot b}{2r^3} + \frac{ra+b}{2r^2 \times r+1}$$

Subtract this latter series from the former, and

$$\frac{a}{1 \cdot r + 1} - \frac{a+b}{r+1 \cdot 2r+1 \cdot 3r+1} + \frac{a+2b}{2r+1 \cdot 3r+1 \cdot 4r+1} - \&c. \dots =$$

$$\frac{2ra-r+1 \cdot 2b}{r^3} \times S + \frac{2r+1 \cdot b-ra}{r^2} - \frac{ra+2b}{2r^2 \times r+1}$$

Let  $r=1$ , and we have

$$\frac{a}{1 \cdot 2 \cdot 3} - \frac{a+b}{2 \cdot 3 \cdot 4} + \frac{a+2b}{3 \cdot 4 \cdot 5} - \&c. \dots = 2a - 4b \times S + \frac{11b-5a}{4}$$

$$\text{If } a=1, b=0, \frac{1}{1 \cdot 2 \cdot 3} - \frac{1}{2 \cdot 3 \cdot 4} + \frac{1}{3 \cdot 4 \cdot 5} - \&c. \dots = 2S - \frac{5}{4};$$

$$a=1, b=2, \frac{1}{1 \cdot 2 \cdot 3} - \frac{3}{2 \cdot 3 \cdot 4} + \frac{5}{3 \cdot 4 \cdot 5} - \&c. \dots = \frac{17}{4} - 6S.$$

Let  $r=3$ , and we have

$$\frac{a}{2 \cdot 4 \cdot 7} - \frac{a+b}{4 \cdot 7 \cdot 10} + \frac{a+2b}{7 \cdot 10 \cdot 13} - \&c. \dots = \frac{6a-8b}{27} \times S + \frac{53b-33a}{216}$$

$$\text{If } a=1, b=0, \frac{1}{2 \cdot 4 \cdot 7} - \frac{1}{4 \cdot 7 \cdot 10} + \frac{1}{7 \cdot 10 \cdot 13} - \&c. \dots = \frac{2}{9} S - \frac{11}{72};$$

$$a=1, b=1, \frac{1}{2 \cdot 4 \cdot 7} - \frac{2}{4 \cdot 7 \cdot 10} + \frac{3}{7 \cdot 10 \cdot 13} - \&c. \dots = \frac{5}{54} - \frac{2}{27} S.$$

If, instead of substituting in prop. 1.  $2b$  and  $a$  for  $n$  and  $m$ , we had substituted two other quantities, as  $2r$  and  $s$ , and then proceeded as above, a series would have been formed, the numerators of whose alternate terms would have formed each a separate arithmetic progression.

If the latter series had been *added* to the former a series would have been formed whose terms would have been all positive; but as I purpose, in the second part of this paper, to give a general method of summing all such series, I shall not stop here to apply this method of investigation.

Cor. 2. In proposition 2. substitute  $a$  for  $m$ , and  $2b$  for  $n$ , and we shall have

$$\frac{a}{r+1 \cdot 2r+1 \cdot 3r+1} + \frac{a+2b}{3r+1 \cdot 4r+1 \cdot 5r+1} + \&c. \dots =$$

$$\frac{2r+1 \cdot b - ra}{r^3} \times S + \frac{ra - 3r+1 \cdot b}{2r^3} + \frac{ra - r-1 \cdot b}{2r^2 \cdot r+1}.$$

Also in prop. 1. write  $a-b$  for  $m$ , and  $2b$  for  $n$ , and there results

$$\frac{a+b}{2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{a+3b}{4r+1 \cdot 5r+1 \cdot 6r+1} + \&c. \dots =$$

$$\frac{ra - 2r+1 \cdot b}{r^3} \times S + \frac{r+1 \cdot b - ra}{2r^3} - \frac{a-b}{r+1 \cdot 2r+1}.$$

Subtract this latter series from the former, and we shall have

$$\frac{a}{r+1 \cdot 2r+1 \cdot 3r+1} - \frac{a+b}{2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{a+2b}{3r+1 \cdot 4r+1 \cdot 5r+1} - \&c. \dots$$

$$= \frac{2r+1 \cdot 2b - 2ra}{r^3} \times S + \frac{ra - 3r+1 \cdot b}{r^3} + \frac{ra - r-1 \cdot b}{2r^2 \cdot r+1} + \frac{a-b}{r+1 \cdot 2r+1}.$$

### P R O P. III.

To find the sum of the infinite series  $\frac{m}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1} +$   
 $\frac{m+n}{2r+1 \cdot 3r+1 \cdot 4r+1 \cdot 5r+1} + \frac{m+2n}{4r+1 \cdot 5r+1 \cdot 6r+1 \cdot 7r+1} + \&c.$

This series resolves itself into

$$\frac{a}{1 \cdot r+1 \cdot 2r+1} - \frac{a+b}{r+1 \cdot 2r+1 \cdot 3r+1} + \frac{a+2b}{2r+1 \cdot 3r+1 \cdot 4r+1} - \&c.$$

for by reduction it becomes

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3ra - b



$$\frac{3ra-b}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1} + \frac{3ra+4r-1 \cdot b}{2r+1 \cdot 3r+1 \cdot 4r+1 \cdot 5r+1} +$$

$$\frac{3ra+8r-1 \cdot b}{4r+1 \cdot 5r+1 \cdot 6r+1 \cdot 7r+1} + \&c.$$

where the numerators are in arithmetic progression, and the denominators the same as in the given series; assume therefore

$$3ra-b=m, 3ra+4r-1 \cdot b=m+n, \text{ hence } b = \frac{n}{4r}, a = \frac{4rm+n}{12r^2};$$

substitute these values into cor. 1. prop. 2. and we have

$$\frac{m}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1} + \frac{m+n}{2r+1 \cdot 3r+1 \cdot 4r+1 \cdot 5r+1} +$$

$$\frac{m+2n}{4r+1 \cdot 5r+1 \cdot 6r+1 \cdot 7r+1} + \&c. \dots = \frac{4rm-3r+2 \cdot n}{6r^4} \times S +$$

$$\frac{3r+1 \cdot n-2rm}{6r^4} - \frac{rm+n}{6r^3 \cdot r+1}.$$

Let  $r=1$ , and we have

$$\frac{m}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{m+n}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{m+2n}{5 \cdot 6 \cdot 7 \cdot 8} + \&c. \dots = \frac{4m-5n}{6} \times S + \frac{7n-5m}{12}.$$

$$\text{If } m=1, n=1, \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{1}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{1}{5 \cdot 6 \cdot 7 \cdot 8} + \&c. \dots = \frac{2}{3}S - \frac{5}{12};$$

$$m=1, n=1, \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{2}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{3}{5 \cdot 6 \cdot 7 \cdot 8} + \&c. \dots = \frac{1}{6}S - \frac{1}{6}S;$$

$$m=7, n=5, \frac{7}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{12}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{17}{5 \cdot 6 \cdot 7 \cdot 8} + \&c. \dots = \frac{1}{2}S;$$

Let  $r=2$ , and we have

$$\frac{m}{1 \cdot 3 \cdot 5 \cdot 7} + \frac{m+n}{5 \cdot 7 \cdot 9 \cdot 11} + \frac{m+2n}{9 \cdot 11 \cdot 13 \cdot 15} + \&c. \dots = \frac{m-n}{12} \times S - \frac{19n-16m}{288}.$$

$$\text{If } m=1, n=3, \frac{1}{1 \cdot 3 \cdot 5 \cdot 7} + \frac{4}{5 \cdot 7 \cdot 9 \cdot 11} + \frac{7}{9 \cdot 11 \cdot 13 \cdot 15} + \&c. \dots = \frac{41}{288} - \frac{1}{6}S;$$

$$m=1, n=0, \frac{1}{1 \cdot 3 \cdot 5 \cdot 7} + \frac{1}{5 \cdot 7 \cdot 9 \cdot 11} + \frac{1}{9 \cdot 11 \cdot 13 \cdot 15} + \&c. \dots = \frac{1}{12}S - \frac{1}{18}.$$

$$m=19, n=16, \frac{19}{1 \cdot 3 \cdot 5 \cdot 7} + \frac{35}{5 \cdot 7 \cdot 9 \cdot 11} + \frac{51}{9 \cdot 11 \cdot 13 \cdot 15} + \&c. \dots = \frac{1}{4}S.$$

Cor. If  $n:m$  as  $4r:3r+2$ , the sum of the series can be accurately had; let therefore  $n=4r$  and  $m=3r+2$ , and we shall have

$$3r+2$$

$$\frac{3r+2}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1} + \frac{7r+2}{2r+1 \cdot 3r+1 \cdot 4r+1 \cdot 5r+1} + \&c. \dots = \frac{1}{2r \cdot r+1}.$$

If  $r=1$ ,  $\frac{5}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{9}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{13}{5 \cdot 6 \cdot 7 \cdot 9} + \&c. \dots = \frac{1}{4}^*$ ;

$r=2$ ,  $\frac{1}{1 \cdot 3 \cdot 5 \cdot 7} + \frac{2}{5 \cdot 7 \cdot 9 \cdot 11} + \frac{3}{9 \cdot 11 \cdot 13 \cdot 15} + \&c. \dots = \frac{1}{96}$ ;

$r=6$ ,  $\frac{5}{1 \cdot 7 \cdot 13 \cdot 19} + \frac{11}{13 \cdot 19 \cdot 25 \cdot 31} + \frac{17}{25 \cdot 31 \cdot 37 \cdot 43} + \&c. \dots = \frac{1}{336}$ .

# P R O P. IV.

To find the sum of the infinite series  $\frac{m}{r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} +$

$$\dots \frac{m+n}{3r+1 \cdot 4r+1 \cdot 5r+1 \cdot 6r+1} + \frac{m+2n}{5r+1 \cdot 6r+1 \cdot 7r+1 \cdot 8r+1} + \&c.$$

This series resolves itself into

$$\frac{a}{r+1 \cdot 2r+1 \cdot 3r+1} - \frac{a+b}{2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{a+2b}{3r+1 \cdot 4r+1 \cdot 5r+1} - \&c.$$

for by reduction it becomes  $\frac{3ra-r+1 \cdot b}{r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} +$

$$\frac{3ra+3r-1 \cdot b}{3r+1 \cdot 4r+1 \cdot 5r+1 \cdot 6r+1} + \frac{3ra+7r-1 \cdot b}{5r+1 \cdot 6r+1 \cdot 7r+1 \cdot 8r+1} + \&c. \text{ where}$$

the denominators are the same as in the given series, and the numerators also in arithmetic progression; put therefore

$$3ra-r+1 \cdot b=m, \quad 3ra+3r-1 \cdot b=m+n, \text{ hence } b=\frac{n}{4r},$$

$$a=\frac{4rm+r+1 \cdot n}{12r^2}, \text{ which, substituted in cor. 2. prop. 2. give}$$

$$\frac{m}{r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{m+n}{3r+1 \cdot 4r+1 \cdot 5r+1 \cdot 6r+1} + \&c. \dots =$$

$$\frac{5r+2 \cdot n-4rm}{6r^4} \times 8 + \frac{2rm-4r+1 \cdot n}{6r^4} + \frac{2rm-r-2 \cdot n}{12 \cdot r^3 \cdot r+1} + \frac{4rm-2r-1 \cdot n}{12r^2 \cdot r+1 \cdot 2r+1}.$$

\* Vide DE MOIVRE'S *Mé. Anal.* pag. 134.

Cor.

Cor. 1. In prop. 3. write  $a$  for  $m$  and  $2b$  for  $n$ , and we have

$$\frac{a}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1} + \frac{a+2b}{2r+1 \cdot 3r+1 \cdot 4r+1 \cdot 5r+1} + \&c. \dots =$$

$$\frac{2ra-3r+2 \cdot b}{3r^4} \times S + \frac{3r+1 \cdot b-ra}{3r^4} - \frac{ra+2b}{6r^3 \cdot r+1}.$$

Also in this prop. write  $a+b$  for  $m$ , and  $2b$  for  $n$ , and we have

$$\frac{a+b}{r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{a+3b}{3r+1 \cdot 4r+1 \cdot 5r+1 \cdot 6r+1} + \&c. \dots =$$

$$\frac{3r+2 \cdot b-2ra}{3r^4} \times S + \frac{ra-3r+1 \cdot b}{3r^4} + \frac{ra+2b}{6r^3 \cdot r+1} + \frac{2ra+b}{6r^2 \cdot r+1 \cdot 2r+1}.$$

subtract this latter series from the former, and we have

$$\frac{a}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1} - \frac{a+b}{r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} + \&c. \dots =$$

$$\frac{4ra-3r+2 \cdot 2b}{3r^4} \times S + \frac{3r+1 \cdot 2b-2ra}{3r^4} - \frac{ra+2b}{3r^3 \cdot r+1} - \frac{2ra+b}{6r^2 \cdot r+1 \cdot 2r+1}.$$

Let  $r=1$ , and we have

$$\frac{a}{1 \cdot 2 \cdot 3 \cdot 4} - \frac{a+b}{2 \cdot 3 \cdot 4 \cdot 5} + \frac{a+2b}{3 \cdot 4 \cdot 5 \cdot 6} - \&c. \dots = \frac{4a-10b}{3} \times S + \frac{83b-32a}{36}.$$

$$\text{If } a=1, b=0, \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} - \frac{1}{2 \cdot 3 \cdot 4 \cdot 5} + \frac{1}{3 \cdot 4 \cdot 5 \cdot 6} - \&c. \dots = \frac{4}{3}S - \frac{8}{9}.$$

$$a=3, b=1, \frac{1}{1 \cdot 2 \cdot 4} - \frac{1}{2 \cdot 3 \cdot 5} + \frac{1}{3 \cdot 4 \cdot 6} - \&c. \dots = \frac{2}{3}S - \frac{13}{36}.$$

Let  $r=3$ , and we have

$$\frac{a}{1 \cdot 4 \cdot 7 \cdot 10} - \frac{a+b}{4 \cdot 7 \cdot 10 \cdot 13} + \frac{a+2b}{7 \cdot 10 \cdot 13 \cdot 16} - \&c. \dots = \frac{12a-22b}{243} \times S + \frac{1027b-156a}{3 \cdot 81 \cdot 56}.$$

$$\text{If } a=1, b=1, \frac{1}{1 \cdot 4 \cdot 7 \cdot 10} - \frac{2}{4 \cdot 7 \cdot 10 \cdot 13} + \frac{3}{7 \cdot 10 \cdot 13 \cdot 16} - \&c. \dots = \frac{871}{3 \cdot 81 \cdot 56} - \frac{10}{3 \cdot 81} S.$$

$$a=4, b=3, \frac{1}{1 \cdot 7 \cdot 10} - \frac{1}{4 \cdot 10 \cdot 13} + \frac{1}{7 \cdot 13 \cdot 16} - \&c. \dots = \frac{13}{72} - \frac{2}{27} S.$$

Cor. 2. If  $a:b$  as  $3r+2:2r$ , the sum of the series can be accurately found; take  $\therefore a=3r+2$ ; and  $b=2r$ , and we shall have

$$\frac{3r+2}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1} - \frac{5r+2}{r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} + \&c. \dots = \frac{1}{r+1 \cdot 2r+1}.$$

If

$$\text{If } r=1, \frac{5}{1 \cdot 2 \cdot 3 \cdot 4} - \frac{7}{2 \cdot 3 \cdot 4 \cdot 5} + \frac{9}{3 \cdot 4 \cdot 5 \cdot 6} - \&c. \dots = \frac{1}{60};$$

$$r=2, \frac{2}{1 \cdot 3 \cdot 5 \cdot 7} - \frac{3}{3 \cdot 5 \cdot 7 \cdot 9} + \frac{4}{5 \cdot 7 \cdot 9 \cdot 11} - \&c. \dots = \frac{1}{15};$$

$$r=3, \frac{11}{1 \cdot 4 \cdot 7 \cdot 10} - \frac{17}{4 \cdot 7 \cdot 10 \cdot 13} + \frac{23}{7 \cdot 10 \cdot 13 \cdot 16} - \&c. \dots = \frac{1}{28}.$$

# P R O P. V.

To find the sum of the infinite series  $\frac{m}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{m+n}{2r+1 \cdot 3r+1 \cdot 4r+1 \cdot 5r+1 \cdot 6r+1} + \frac{m+2n}{4r+1 \cdot 5r+1 \cdot 6r+1 \cdot 7r+1 \cdot 8r+1} + \&c.$

This series resolves itself into  $\frac{a}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1} - \frac{a+b}{r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{a+2b}{2r+1 \cdot 3r+1 \cdot 4r+1 \cdot 5r+1} - \&c.$ ; for by reduction this series becomes  $\frac{4ra-b}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{4ra+b-1 \cdot b}{2r+1 \cdot 3r+1 \cdot 4r+1 \cdot 5r+1 \cdot 6r+1} + \frac{4ra+12r-1 \cdot b}{4r+1 \cdot 5r+1 \cdot 6r+1 \cdot 7r+1 \cdot 8r+1} + \&c.$

where the numerators are in arithmetic progression, and the denominators the same as in the given series; assume therefore  $4ra-b=m$ ,  $4ra+6r-1 \cdot b=m+n$ , hence  $b = \frac{n}{6r}$ ,  $a = \frac{6rm+n}{24r^2}$ , which values being substituted in cor. 1. prop. 4. give

$$\frac{m}{1 \cdot r+1 \cdot 2r+1 \cdot 3r+1 \cdot 4r+1} + \frac{m+n}{2r+1 \cdot 3r+1 \cdot 4r+1 \cdot 5r+1 \cdot 6r+1} + \&c. \dots = \frac{2rm-2r+1 \cdot n}{6r^3} \times S + \frac{4r+1 \cdot n-2rm}{12r^3} - \frac{6rm+9n}{72 \cdot r+1 \cdot r+1} - \frac{2rm+n}{24r^3 \cdot r+1 \cdot 2r+1}.$$

Let  $r=1$ , and we have

$$\frac{m}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \frac{m+n}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7} + \frac{m+2n}{5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} + \&c. \dots = \frac{2m-n}{6} \times S + \frac{25n-16m}{72}.$$

If

$$\text{If } m=1, n=0, \frac{1}{1.2.3.4.5} + \frac{1}{3.4.5.6.7} + \frac{1}{5.6.7.8.9} + \&c. \dots = \frac{1}{3}S - \frac{2}{9};$$

$$m=1, n=1, \frac{1}{1.2.3.4.5} + \frac{2}{3.4.5.6.7} + \frac{3}{5.6.7.8.9} + \&c. \dots = \frac{1}{8} - \frac{1}{6}S;$$

$$m=4, n=2, \frac{1}{1.2.3.5} + \frac{1}{3.4.5.7} + \frac{1}{5.6.7.9} + \&c. \dots = \frac{1}{3}S - \frac{7}{36}.$$

$$m=25, n=16, \frac{25}{1.2.3.4.5} + \frac{41}{3.4.5.6.7} + \frac{57}{5.6.7.8.9} + \&c. \dots = \frac{1}{3}S.$$

$$\text{Let } r \neq 2, \text{ and we have } \frac{m}{1.3.5.7.9} + \frac{m+n}{5.7.9.11.13} + \frac{m+2n}{9.11.13.15.17} + \&c. \dots = \frac{4m-5n}{192} \times S + \frac{59n-44m}{24.120}.$$

$$\text{If } m=1, n=0, \frac{1}{1.3.5.7.9} + \frac{1}{5.7.9.11.13} + \frac{1}{9.11.13.15.17} + \&c. \dots = \frac{1}{48}S - \frac{11}{24.306}.$$

$$m=3, n=2, \frac{1}{1.5.7.9} + \frac{1}{5.9.11.13} + \frac{1}{9.13.15.17} + \&c. \dots = \frac{1}{96}S - \frac{7}{24.60}.$$

Cor. If  $n:m$  as  $2r:2r+1$ , the sum of the series can be accurately found; assume therefore  $n=2r$ ,  $m=2r+1$ , and we have

$$\frac{1}{1.r+1.3r+1.4r+1} + \frac{1}{2r+1.3r+1.5r+1.6r+1} + \&c. \dots = \frac{1}{6.r.r+1.2r+1}.$$

$$\text{If } r=1, \frac{1}{1.2.4.5} + \frac{1}{3.4.6.7} + \frac{1}{5.6.8.9} + \&c. \dots = \frac{1}{36};$$

$$r=3, \frac{1}{1.4.10.13} + \frac{1}{7.10.16.19} + \frac{1}{13.16.22.25} + \&c. \dots = \frac{1}{504}.$$

Having thus far explained the method of summation of such series as I proposed to treat of in the first part of this paper, I trust it is not necessary to say any thing further, as the same method of proceeding will manifestly continue the series to any proposed number of factors in the denominator; I shall therefore conclude with pointing out a remarkable property of those series whose sum can be accurately found: that when the number of factors in the denominator is *even*, the numerator is always equal to the sum of the two

middle factors; and when the number of factors be odd, the numerator will be equal to the middle factor, and consequently will take it out of the denominator, and leave a series whose numerators are unity, and whose denominators want the middle factor.

The method of summation of series here made use of may also be applied in investigating the sums of a great variety of other series; but as a further application of this method would carry us beyond the limits to which this paper must be confined, I shall re-assume the subject at some future opportunity, and proceed immediately to the second part.

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## P A R T II.

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### P R O P.

*To find the sum of the infinite series*  $\frac{p}{n \cdot n+m \dots n+rm} + \frac{q}{n+m \dots n+r+1 \cdot m} + \frac{s}{n+2m \dots n+r+2 \cdot m} + \&c.$  *when the last differences of the numerators become equal to nothing.*

Assume  $a + nb + n \cdot \overline{n+m} \cdot c + n \cdot \overline{n+m} \cdot \overline{n+2m} \cdot d + \&c.$  to any number ( $r$ ) of terms; then, if for  $n$  we write  $n+m, n+2m, n+3m, \&c.$  successively, there will result a series of quantities

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whose

whose  $r$ th difference is  $= 0$ ; substitute, therefore, this series of quantities for  $p, q, s, \&c.$  respectively, and the given series becomes

$$\frac{a + nb + n \cdot n + m \cdot c + \&c.}{n \cdot n + m \dots n + rm} + \frac{a + n + m \cdot b + n + m \cdot n + 2m \cdot c + \&c.}{n + m \dots n + r + 1 \cdot m} +$$

$$\frac{a + n + 2m \cdot b + n + 2m \cdot n + 3m \cdot c + \&c.}{n + 2m \dots n + r + 2 \cdot m} + \&c.$$

which manifestly resolves itself into the following series

$$\frac{a}{n \cdot n + m \dots n + rm} + \frac{a}{n + m \dots n + r + 1 \cdot m} + \frac{a}{n + 2m \dots n + r + 2 \cdot m} + \&c.$$

$$\frac{b}{n + m \dots n + rm} + \frac{b}{n + 2m \dots n + r + 1 \cdot m} + \frac{b}{n + 3m \dots n + r + 2 \cdot m} + \&c.$$

$$\frac{c}{n + 2m \dots n + rm} + \frac{c}{n + 3m \dots n + r + 1 \cdot m} + \frac{c}{n + 4m \dots n + r + 2 \cdot m} + \&c.$$

$\&c. \qquad \qquad \&c. \qquad \qquad \&c.$

where the number of series is  $r$ , the sum of each of which being taken by a well known rule, the sum of the given series becomes

$$\frac{a}{n \cdot n + m \dots n + r - 1 \cdot m \cdot m \cdot r} + \frac{b}{n + m \dots n + r - 1 \cdot m \cdot m \cdot r - 1} +$$

$$\frac{c}{n + 2m \dots n + r - 1 \cdot m \cdot m \cdot r - 2} + \&c.$$

where the law of continuation is manifest.

CASE I. To find the sum of the infinite series  $\frac{3}{1 \cdot 2 \cdot 3 \cdot 4} +$

$$\frac{6}{2 \cdot 3 \cdot 4 \cdot 5} + \frac{10}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{15}{4 \cdot 5 \cdot 6 \cdot 7} + \&c.$$

Here  $n = 1$ ,  $m = 1$ ,  $r = 3$ , and the third differences become  $= 0$ ; therefore  $a + b + 2c = 3$ ,  $a + 2b + 6c = 6$ ,  $a + 3b + 12c = 10$ , consequently  $a = 1$ ,  $b = 1$ ,  $c = \frac{1}{2}$ , and therefore the sum sought will be  $\frac{1}{1 \cdot 2 \cdot 3 \cdot 3} + \frac{1}{2 \cdot 3 \cdot 2} + \frac{1}{2 \cdot 3} = \frac{11}{36}$ .

CASE

CASE 2. To find the sum of the infinite series  $\frac{1 \cdot 2}{1 \cdot 3 \cdot 5 \cdot 7} + \frac{2 \cdot 3}{3 \cdot 5 \cdot 7 \cdot 9} + \frac{3 \cdot 4}{5 \cdot 7 \cdot 9 \cdot 11} + \&c.$

In this case  $n = 1, m = 2, r = 3$ , and the 3d differences become  $= 0$ ; therefore  $a + b + 3c = 2, a + 3b + 15c = 6, a + 5b + 35c = 12$ , consequently  $a = \frac{1}{4}, b = \frac{1}{2}, c = \frac{1}{4}$ , and hence the sum of the required series becomes  $\frac{3}{1 \cdot 3 \cdot 5 \cdot 2 \cdot 3 \cdot 4} + \frac{1}{3 \cdot 5 \cdot 2 \cdot 2 \cdot 2} + \frac{1}{5 \cdot 2 \cdot 4} = \frac{1}{24}.$

CASE 3. To find the sum of the infinite series  $\frac{1}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8} + \frac{2}{4 \cdot 5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} + \frac{4}{5 \cdot 6 \cdot 7 \cdot 8 \cdot 9 \cdot 10} + \frac{12}{6 \cdot 7 \cdot 8 \cdot 9 \cdot 10 \cdot 11} + \frac{31}{7 \cdot 8 \cdot 9 \cdot 10 \cdot 11 \cdot 12} + \&c.$

Here  $n = 3, m = 1, r = 5$ , and the 4th differences become  $= 0$ ; therefore  $a + 3b + 12c + 60d = 1, a + 4b + 20c + 120d = 2, a + 5b + 30c + 210d = 4, a + 6b + 42c + 336d = 12$ , consequently  $a = -54, b = 47, c = -12, d = \frac{1}{6}$ , therefore the sum of the given series is  $\frac{-46}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8} + \frac{47}{4 \cdot 5 \cdot 6 \cdot 7 \cdot 8} - \frac{12}{5 \cdot 6 \cdot 7 \cdot 8} + \frac{5}{6 \cdot 7 \cdot 8} = \frac{61}{50400}.$

CASE 4. To find the sum of the infinite series  $\frac{1}{1 \cdot 4 \cdot 7 \cdot 10} + \frac{9}{4 \cdot 7 \cdot 10 \cdot 13} + \frac{25}{7 \cdot 10 \cdot 13 \cdot 16} + \&c.$

Here  $n = 1, m = 3, r = 3$ , and the 3d differences are  $= 0$ ; therefore  $a + b + 4c = 1, a + 4b + 28c = 9, a + 7b + 70c = 25$ , consequently  $a = \frac{1}{9}, b = -\frac{8}{9}, c = \frac{4}{9}$ ;  $\therefore$  the sum of the given series will be  $\frac{1}{1 \cdot 4 \cdot 7 \cdot 3 \cdot 3 \cdot 9} - \frac{8}{4 \cdot 7 \cdot 3 \cdot 2 \cdot 9} + \frac{4}{7 \cdot 3 \cdot 9} = \frac{37}{2268}.$



CASE 5. To find the Sum of the infinite Series  $\frac{1}{1 \cdot 3 \cdot 5 \cdot 7 \cdot 9} + \frac{4}{3 \cdot 5 \cdot 7 \cdot 9 \cdot 11} + \frac{10}{5 \cdot 7 \cdot 9 \cdot 11 \cdot 13} + \frac{20}{7 \cdot 9 \cdot 11 \cdot 13 \cdot 15} + \dots$

In this case  $n = 1$ ,  $m = 2$ ,  $r = 4$ , and the 4th differences become  $= 0$ ; therefore  $a + b + 3c + 15d = 1$ ,  $a + 3b + 15c + 105d = 4$ ,  $a + 5b + 35c + 315d = 10$ ,  $a + 7b + 63c + 693d = 20$ , consequently  $a = \frac{1}{16}$ ,  $b = \frac{3}{16}$ ,  $c = \frac{1}{16}$ ,  $d = \frac{1}{16}$ , and hence the sum of the given series becomes  $\frac{5}{1 \cdot 3 \cdot 5 \cdot 7 \cdot 2 \cdot 4 \cdot 16} + \frac{3}{3 \cdot 5 \cdot 7 \cdot 2 \cdot 3 \cdot 16} + \frac{1}{5 \cdot 7 \cdot 2 \cdot 2 \cdot 16} + \frac{1}{7 \cdot 2 \cdot 48} = \frac{1}{384}$ .

This proposition may also be applied to find the sum of all those series whose numerators being unity, the denominators shall be deficient by any number of corresponding terms, however taken: for as the product of all such factors must form a progression, whose differences will become equal to nothing, if such products be assumed for the numerators of the given series having its factors compleated, another series will be formed equal to the given series, whose sum can be found by this proposition.

CASE 1. To find the sum of the infinite Series  $\frac{1}{1 \cdot 2 \cdot 4 \cdot 6} + \frac{1}{2 \cdot 3 \cdot 5 \cdot 7} + \frac{1}{3 \cdot 4 \cdot 6 \cdot 8} + \dots$

By completing the factors in the denominators, and multiplying the numerators by the same quantities the given series becomes  $\frac{15}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6} + \frac{24}{2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7} + \frac{35}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8} + \dots$  in which case  $n = 1$ ,  $m = 1$ ,  $r = 5$ , and the 3d differences become  $= 0$ ;

$= 0$ ; therefore  $a + b + 2c = 15$ ,  $a + 2b + 6c = 24$ ,  $a + 3b + 12c = 35$ , consequently  $a = 8$ ,  $b = 5$ ,  $c = 1$ , and therefore the sum of the series required is  $\frac{8}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 5} + \frac{5}{2 \cdot 3 \cdot 4 \cdot 5 \cdot 4} + \frac{1}{3 \cdot 4 \cdot 5 \cdot 3} = \frac{211}{7200}$ .

CASE 2. To find the sum of the infinite series  $\frac{1}{1 \cdot 5 \cdot 11} + \frac{1}{3 \cdot 7 \cdot 13} + \frac{1}{5 \cdot 9 \cdot 15} + \&c.$

This series, when completed, becomes  $\frac{189}{1 \cdot 3 \cdot 5 \cdot 7 \cdot 9 \cdot 11} + \frac{495}{3 \cdot 5 \cdot 7 \cdot 9 \cdot 11 \cdot 13} + \frac{1001}{5 \cdot 7 \cdot 9 \cdot 11 \cdot 13 \cdot 15} + \frac{1755}{7 \cdot 9 \cdot 11 \cdot 13 \cdot 15 \cdot 17} + \&c.$  where  $n = 1$ ,  $m = 2$ ,  $r = 5$ , and the 4th differences are  $= 0$ ; therefore  $a + b + 3c + 15d = 189$ ,  $a + 3b + 15c + 105d = 495$ ,  $a + 5b + 35c + 315d = 1001$ ,  $a + 7b + 63c + 693d = 1755$ , consequently  $a = 96$ ,  $b = 48$ ,  $c = 10$ ,  $d = 1$ ; and hence the sum of the given series is  $\frac{96}{1 \cdot 3 \cdot 5 \cdot 7 \cdot 9 \cdot 2 \cdot 5} + \frac{48}{3 \cdot 5 \cdot 7 \cdot 9 \cdot 2 \cdot 4} + \frac{10}{5 \cdot 7 \cdot 9 \cdot 2 \cdot 3} + \frac{1}{7 \cdot 9 \cdot 2 \cdot 2} = \frac{487}{18900}$ .

CASE 3. To find the sum of the infinite series  $\frac{1}{1 \cdot 4 \cdot 13 \cdot 16} + \frac{1}{4 \cdot 7 \cdot 16 \cdot 19} + \frac{1}{7 \cdot 10 \cdot 19 \cdot 22} + \&c.$

This series resolves itself into  $\frac{70}{1 \cdot 4 \cdot 7 \cdot 10 \cdot 13 \cdot 16} + \frac{130}{4 \cdot 7 \cdot 10 \cdot 13 \cdot 16 \cdot 19} + \frac{208}{7 \cdot 10 \cdot 13 \cdot 16 \cdot 19 \cdot 22} + \&c.$  where  $n = 1$ ,  $m = 3$ ,  $r = 5$ , and the 3d differences  $= 0$ ; therefore  $a + b + 4c = 70$ ,  $a + 4b + 28c = 130$ ,  $a + 7b + 70c = 208$ , from whence  $a = 54$ ,  $b = 12$ ,  $c = 1$ ; therefore the sum of the given series is

$$\frac{54}{1 \cdot 4 \cdot 7 \cdot 10 \cdot 13 \cdot 3 \cdot 5} + \frac{12}{4 \cdot 7 \cdot 10 \cdot 13 \cdot 3 \cdot 4} + \frac{1}{7 \cdot 10 \cdot 13 \cdot 3 \cdot 3} = \frac{227}{5800}.$$

By this proposition we may also investigate the sum of the series when there are any number of deficient terms in the denominators, and where the last differences of the numerators become equal to nothing; for if the factors in the denominators be completed, and the numerators be multiplied by the same quantities, their differences will still become equal to nothing.

CASE 1. To find the sum of the infinite series  $\frac{1}{1 \cdot 3 \cdot 4 \cdot 0} + \frac{3}{2 \cdot 4 \cdot 5 \cdot 7} + \frac{6}{3 \cdot 5 \cdot 6 \cdot 8} + \frac{10}{4 \cdot 6 \cdot 7 \cdot 9} + \frac{15}{5 \cdot 7 \cdot 8 \cdot 10} + \&c.$

This series, by completing the factors in the denominators and multiplying the numerators by the same quantities, becomes

$$\frac{10}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 0} + \frac{54}{2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7} + \frac{168}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8} + \&c. \text{ in which case } n=1, m=1, r=5, \text{ and as the 5th differences are } = 0;$$

$$\begin{aligned} \therefore a + b + 2c + 6d + 24e &= 10, \quad a + 2b + 6c + 24d + 120e = 54, \\ a + 3b + 12c + 60d + 360e &= 168, \quad a + 4b + 20c + 120d + 840e = 400, \\ a + 5b + 30c + 210d + 1680e &= 810, \text{ from whence } a=0, \quad b=0, \\ c=-1, \quad d=0, \quad e=\frac{1}{2}, \text{ consequently the sum of the given series is} \\ &= -\frac{1}{3 \cdot 4 \cdot 5 \cdot 3} + \frac{1}{5 \cdot 2} = \frac{17}{180}. \end{aligned}$$

CASE 2. To find the sum of the infinite series  $\frac{1}{1 \cdot 3 \cdot 7 \cdot 9} + \frac{5}{3 \cdot 5 \cdot 9 \cdot 11} + \frac{11}{5 \cdot 7 \cdot 11 \cdot 13} + \frac{19}{7 \cdot 9 \cdot 13 \cdot 15} + \&c.$

By proceeding as before this series becomes  $\frac{5}{1 \cdot 3 \cdot 5 \cdot 7 \cdot 9} +$

$\frac{35}{3 \cdot 5 \cdot 7 \cdot 9 \cdot 11} + \frac{99}{5 \cdot 7 \cdot 9 \cdot 11 \cdot 13} + \&c.$  where  $n=1, m=2, r=4$ ,  
 and the 4th differences = 0; therefore  $a+b+3c+15d=5$ ,  
 $a+3b+15c+105d=35$ ,  $a+5d+35c+315d=99$ ,  
 $a+7b+63c+693d=209$ , consequently  $a=-1, b=\frac{1}{4}, c=\frac{1}{4},$   
 $d=\frac{1}{4}$ , therefore the sum of the given series =  $\frac{-1}{1 \cdot 3 \cdot 5 \cdot 7 \cdot 2 \cdot 4} +$   
 $\frac{3}{3 \cdot 5 \cdot 7 \cdot 2 \cdot 3 \cdot 4} + \frac{1}{5 \cdot 7 \cdot 2 \cdot 2 \cdot 2} + \frac{1}{7 \cdot 2 \cdot 4} = \frac{3}{140}.$

By a method similar to that made use of in this proposition may any number of factors be taken from the denominators of those series delivered in part the first, and also from a great variety of others; but as the examples here given must be sufficient to point out the method of proceeding in all other cases, we will proceed to the third part.

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## P A R T III.

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THE sum of every converging infinite series, whose terms ultimately become equal to nothing, may always be exhibited by the sum of another series formed by collecting two or more terms of the former series into one. This is not true, however, where the terms of the infinite series continually diverge, or converge to any assignable quantity, and.

and are affected with the signs  $+$ ,  $-$ , alternately: for instance, the series  $\frac{1}{1} - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \&c.$  if we collect two terms into one, beginning at the first term, will become  $-\frac{1}{2 \cdot 3} - \frac{1}{4 \cdot 5} - \frac{1}{6 \cdot 7} + \&c.$  If we begin at the second term it becomes  $\frac{1}{1 \cdot 2} + \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 6} + \&c.$ ; neither of which gives the sum of the assumed series; but in this, and every other case of the like nature, a correction will be necessary: to determine the value of which, and from whence the necessity thereof arises, is the subject of this third part.

### L E M M A.

If  $r$  be any quantity whatever, then will  $\frac{1}{2r} = \frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \frac{1}{r} + \&c.$  ad infinitum.

For  $\frac{1}{2r} = \frac{1}{r+r} =$  (by common division)  $\frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \&c.$  ad infinitum.

Cor. 1. Hence  $-\frac{1}{2r} = -\frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \&c.$  ad infinitum.

Cor. 2. Hence also  $\frac{x}{2v} = \frac{x}{v} - \frac{x}{v} + \frac{x}{v} - \frac{x}{v} + \&c.$  ad infinitum;

and  $-\frac{x}{2v} = -\frac{x}{v} + \frac{x}{v} - \frac{x}{v} + \frac{x}{v} - \&c.$  ad infinitum.

### P R O P. I.

If  $\frac{r^n + m}{n}$  be the general term of a series formed by writing for  $n$  any series of numbers in arithmetic progression, and whose signs are alternately  $+$  and  $-$ ; then if a series be formed by collecting

collecting two terms into one, beginning at the first term, the sum of the series thence arising will be less than the sum of the given series by  $\frac{1}{2r}$ . If a series be formed by beginning at the second Term, the sum thereof will be greater than the sum of the given series by  $\frac{1}{2r}$ .

For let  $\frac{n}{rn+m} - \frac{n+a}{n+a.r+m}$  be any two successive terms of the series, which, if we begin to collect at the first term (the first term being +) will be two terms to be collected into one, and which will therefore give  $\frac{-am}{rn+m \times n+a.r+m}$  for a general term of the resulting series. Let us now make  $n$  infinite, and then the denominator of this term becomes infinite, and the numerator finite; therefore the terms of this latter series at an infinite distance becoming infinitely small, the series will there terminate. Now, by making  $n$  infinite in the given series, the two successive general terms at an infinite distance become  $\frac{1}{r} - \frac{1}{r}$ ; consequently *this* series is still continued after the *other* terminates; and the terms of such a continuation will be (as they begin with  $\frac{n}{rn+m} - \frac{n+a}{n+a.r+m}$  by making  $n$  infinite)  $\frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \&c.$  which will also be continued ad infin. and whose sum by the lemma is  $\frac{1}{2r}$ ; consequently the given series exceeds that which is formed by collecting two terms into one, beginning at the first, by  $\frac{1}{2r}$ ; hence the sum of the *latter* series  $+ \frac{1}{2r}$  will be equal to the sum of the *former*. If we begin to

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collect

collect at the second term, then will  $-\frac{n}{rn+m} + \frac{n+a}{n+a.r+m}$  be the two successive general terms of the given series to be collected into one; consequently the continuation of the given series when  $n$  becomes infinite will be  $-\frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \&c.$  ad infinitum, whose sum, by cor. 1. to the lem. is  $-\frac{1}{2r}$ ; in this case, therefore, the sum of the given series is less than the sum of the series formed by collecting two terms into one, beginning at the second term, by  $\frac{1}{2r}$ ; hence the sum of the *latter* series  $-\frac{1}{2r}$  will be equal to the sum of the *former*.

CASE 1. Let the given series be  $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} - \frac{4}{5} + \&c.$

Here  $r=1$ ,  $n=1, 2, 3, 4, \&c.$  and  $m=1$ . Now, if we begin to collect at the first term, the series resolves itself into  $-\frac{1}{2 \cdot 3} - \frac{1}{4 \cdot 5} - \frac{1}{6 \cdot 7} - \&c.$  and the correction, to be *added*, being  $\frac{1}{2}$ , we have  $-\frac{1}{2 \cdot 3} - \frac{1}{4 \cdot 5} - \frac{1}{6 \cdot 7} - \&c. + \frac{1}{2}$  for the sum of the given series. Now  $-\frac{1}{2 \cdot 3} - \frac{1}{4 \cdot 5} - \frac{1}{6 \cdot 7} - \&c.$  is well known to be equal to  $-1 + \text{hyp. log. of } 2$ ; consequently the sum of the given series is  $= -\frac{1}{2} + \text{hyp. log. of } 2$ .

If we begin to collect at the second term, the series becomes  $\frac{1}{1 \cdot 2} + \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 6} + \&c.$  and the correction, to be *subtracted*, being  $\frac{1}{2}$ , we have  $\frac{1}{1 \cdot 2} + \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 6} + \&c. - \frac{1}{2}$  for the sum of the given series;

series; but  $\frac{1}{1 \cdot 2} + \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 6} + \&c.$  is equal to the hyp. log. of 2; therefore the sum of the given series is  $= -\frac{1}{2} + \text{hyp. log. of } 2$ , the same as before.

CASE 2. Let the given series be  $\frac{1}{3} - \frac{2}{5} + \frac{3}{7} - \frac{4}{9} + \&c.$

Here  $r=2$ ,  $n=1, 2, 3, 4, \&c.$   $m=1$ . Now, if we begin to collect at the second term, the series becomes  $\frac{1}{1 \cdot 3} + \frac{1}{5 \cdot 7} + \frac{1}{9 \cdot 11} + \&c.$  and the correction, to be subtracted, being  $\frac{1}{4}$ , we have  $\frac{1}{1 \cdot 3} + \frac{1}{5 \cdot 7} + \frac{1}{9 \cdot 11} + \&c. - \frac{1}{4}$  for the sum of the given series; but  $\frac{1}{1 \cdot 3} + \frac{1}{5 \cdot 7} + \frac{1}{9 \cdot 11} + \&c.$  is equal to a circular arc (A) of  $22^\circ \frac{1}{2}$ , whose radius is unity; therefore the sum of the given series  $= A - \frac{1}{4}$ .

## P R O P. II.

If  $\frac{x+nx}{w+nv}$  be the general term of a series formed by writing for  $n$ , any series of numbers in arithmetic progression, and whose terms are alternately + and -; then if a series be formed by collecting two terms into one, beginning at the first term, the sum of the series thence arising will be less than the sum of the given series by  $\frac{z}{2v}$ . If a series be formed by beginning at the second term, the sum thereof will be greater than the sum of the given series by  $\frac{z}{2v}$ .

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For



For let  $\frac{x+nx}{w+nv} - \frac{x+\overline{n+a}.z}{w+\overline{n+a}.v}$  be any two successive terms of the given series, which, if we begin to collect at the first term, will be the two general terms to be collected into one, and will therefore give  $\frac{axv-awz}{w+nv \times w+\overline{n+a}.v}$  for a general term of the resulting series.

Let us now make  $n$  infinite and then this term will vanish, and consequently the resulting series will terminate at an infinite distance. Now, by making  $n$  infinite in the given series, the two successive terms (as they begin with  $\frac{x+nx}{w+nv} - \frac{x+\overline{n+a}.z}{w+\overline{n+a}.v}$  by making  $n$  infinite) become  $\frac{z}{v} - \frac{z}{v}$ ; this series, therefore, is still continued after the other terminates; and the terms of such a continuation will be  $\frac{z}{v} - \frac{z}{v} + \frac{z}{v} - \frac{z}{v} + \&c.$  ad infinitum, and whose sum by cor. 2. to the lem. is  $\frac{z}{2v}$ ; consequently the given series exceeds that which is formed by collecting two terms into one, beginning at the first, by  $\frac{z}{2v}$ ; hence the sum of the *latter* series  $+ \frac{z}{2v}$  will be equal to the sum of the *former*. Now, if we begin to collect at the second term, then will  $-\frac{x+nx}{w+nv} + \frac{x+\overline{n+a}.z}{w+\overline{n+a}.v}$  be two general terms of the given series to be collected into one; consequently the continuation of the given series, when  $n$  becomes infinite, will be  $-\frac{z}{v} + \frac{z}{v} - \frac{z}{v} + \frac{z}{v} - \&c.$  ad infinitum, whose sum by cor. 2. to the lem. is  $-\frac{z}{2v}$ ; in this case, therefore, the sum of the given series is less than the sum of the series formed by collecting two terms into one, beginning at the second term, by

z

$\frac{z}{2v}$ ; hence the sum of the *latter* series  $-\frac{z}{2v}$  will be equal to the sum of the *former*.

CASE 1. Let the given series be  $\frac{7}{3} - \frac{11}{5} + \frac{15}{7} - \frac{19}{9} + \&c.$

Here  $x=3$ ,  $z=2$ ,  $w=1$ ,  $v=1$ ,  $n=2, 4, 6, 8, \&c.$  Now, if we begin to collect at the first term, the series becomes  $\frac{2}{3 \cdot 5} + \frac{2}{7 \cdot 9} + \frac{2}{11 \cdot 13} + \&c.$  and the correction, to be *added*, being 1, we have  $\frac{2}{3 \cdot 5} + \frac{2}{7 \cdot 9} + \frac{2}{11 \cdot 13} + \&c. + 1$  for the sum of the given series; but if  $A =$  a circular arc of  $45^\circ$  whose radius is unity, it is well known that  $\frac{2}{3 \cdot 5} + \frac{2}{7 \cdot 9} + \frac{2}{11 \cdot 13} + \&c. = 1 - A$ ; therefore the sum of the given series is  $2 - A$ .

CASE 2. Let the given series be  $\frac{16}{1} - \frac{27}{2} + \frac{38}{3} - \frac{49}{4} + \&c.$

Here  $w=1$ ,  $v=1$ ,  $x=16$ ,  $z=11$ ,  $n=0, 1, 2, 3, \&c.$  Now, if we begin to collect at the first term, the series becomes  $\frac{5}{1 \cdot 2} + \frac{5}{3 \cdot 4} + \frac{5}{5 \cdot 6} + \&c.$  and the correction, to be *added*, being  $\frac{11}{2}$ , we have  $\frac{5}{1 \cdot 2} + \frac{5}{3 \cdot 4} + \frac{5}{5 \cdot 6} + \&c. + \frac{11}{2}$  for the sum of the given series; but  $\frac{5}{1 \cdot 2} + \frac{5}{3 \cdot 4} + \frac{5}{5 \cdot 6} + \&c.$  is equal to  $5 \times \text{hyp. log. of } 2$ , consequently the sum of the given series is equal to  $\frac{11}{2} + 5 \times \text{hyp. log. of } 2$ .

Because  $\frac{axv - awx}{w + nv \times w + n + a \cdot v}$ , the general term of the series.

formed

formed by reducing two terms into one, has its numerator independent of the value of,  $n$ , it is manifest, that the numerators of that series will be all equal. Now, if a series be assumed, the numerators of whose terms are unity, and in every other respect the same as the series in this proposition, that is, if

$\frac{1}{w+nv} - \frac{1}{w+n+a \cdot v}$  be two successive terms of a series, it is manifest,

that if every two terms of this series be reduced into one, the general term of the resulting series will be

$\frac{-av}{w+nv \times w+n+a \cdot v}$ , where the numerator is a constant quantity

$-av$ ; consequently the sum of the series whose general term is

$\frac{avx - awz}{w+nv \times w+n+a \cdot v}$  is to the sum of the series whose general term

is  $\frac{-av}{w+nv \times w+n+a \cdot v}$  as  $vx - wz$  to  $-v$ , or in a given ratio;

whenever, therefore, the sum of the latter series can be found, the sum of the former can be found, and consequently, after proper correction, the sum of the series in this proposition can be found.

Hence, therefore, in the two cases given above in whatever arithmetic progression the numerators may proceed, the sum of the former can always be expressed by circular arcs, and the latter by the hyp. log. of 2.

Hence also, as it appears from lem. 1. part the first, that the sum of the series  $\frac{1}{1} - \frac{1}{r+1} + \frac{1}{2r+1} - \frac{1}{3r+1} + \&c.$  can always be expressed by circular arcs and logarithms, it is manifest, that if the numerators form any arithmetic progression, the sum of such series may be found by this proposition, and will always be exhibited by circular arcs and logarithms.

Besides

Besides the series contained in the foregoing propositions, a great variety of other series might be produced where a correction is necessary, after collecting two terms into one, in order to exhibit the true value of the given series. As the proper correction, however, may always be found from the principles delivered in the above propositions, that is, by considering what the terms of the given series become at an infinite distance, I shall only add one or two instances more, and conclude what I at present intend to offer on this subject.

**EX. 1.** *Let it be required to find the sum of the infinite series*

$$\frac{3 \cdot 4}{1 \cdot 2} - \frac{4 \cdot 5}{2 \cdot 3} + \frac{5 \cdot 6}{3 \cdot 4} - \frac{6 \cdot 7}{4 \cdot 5} + \&c.$$

This, by resolving two terms into one, becomes  $\frac{16}{1 \cdot 2 \cdot 3} + \frac{24}{3 \cdot 4 \cdot 5} + \frac{32}{5 \cdot 6 \cdot 7} - \&c.$ ; and as the terms of the given series continually approach to unity, the correction, to be *added*, is  $\frac{1}{2}$ , consequently  $\frac{16}{1 \cdot 2 \cdot 3} + \frac{24}{3 \cdot 4 \cdot 5} + \frac{32}{5 \cdot 6 \cdot 7} - \&c. + \frac{1}{2}$  is equal to the sum of the given series; but by prop. 1. part I. the sum of the series  $\frac{16}{1 \cdot 2 \cdot 3} + \frac{24}{3 \cdot 4 \cdot 5} + \frac{32}{5 \cdot 6 \cdot 7} + \&c.$  is equal to  $8S - 2$  ( $S$  being the hyp. log. 2.) consequently the sum of the given series is  $8S - 1\frac{1}{2}$ .

**EX. 2.** *Let it be required to find the sum of the infinite series*

$$\frac{1 \cdot 2}{1 \cdot 3} - \frac{2 \cdot 3}{3 \cdot 5} + \frac{3 \cdot 4}{5 \cdot 7} - \frac{4 \cdot 5}{7 \cdot 9} + \&c.$$

This series, by resolving two terms into one, becomes  $\frac{4}{1 \cdot 3 \cdot 5} + \frac{8}{5 \cdot 7 \cdot 9} + \frac{12}{9 \cdot 11 \cdot 13} + \&c.$  and as the terms of the given series

series continually approach to  $\frac{1}{4}$ , the correction, to be *added*, will be  $\frac{1}{8}$ , therefore  $\frac{4}{1 \cdot 3 \cdot 5} + \frac{8}{5 \cdot 7 \cdot 9} + \frac{12}{9 \cdot 11 \cdot 13} + \&c. + \frac{1}{8}$  is = to the sum of the given series; but by prop. 1. part I. the sum of  $\frac{4}{1 \cdot 3 \cdot 5} + \frac{8}{5 \cdot 7 \cdot 9} + \frac{12}{9 \cdot 11 \cdot 13} + \&c.$  is equal to  $\frac{1}{4}S + \frac{1}{8}$  (S being a circular arc of  $45^\circ$ , whose radius is unity) hence the sum of the given series is  $\frac{1}{4}S + \frac{1}{4}$ .

This method is not only applicable to those cases, where the given series resolves itself into another, whose sum is either accurately known or can be expressed by circular arcs and logarithms, but also to those cases where we want to approximate to the value of the given series, as it must, in general, be necessary first to render the terms of the series converging, by collecting two into one, before the operation of approximation begins, and consequently a correction of this latter is necessary in order to exhibit the value of the given series.

XXVI. *A new Method of finding the equal Roots of an Equation, by Division. By the Rev. John Hellins, Curate of Constantine, in Cornwall; communicated by Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read June 20, 1782.

THE following theorems are a production of juvenile years. They were invented about twelve years ago, when algebra was my favourite study; and one of them (the first) was published as a specimen of this method of extracting the equal roots of an equation about ten years ago. Since that time my avocations have left me but very little leisure for improving any invention of this kind. These theorems, then, are in their crude state; however, such as they are, I flatter myself, they will afford an easier solution of equations that have equal roots than is generally known, and be acceptable to the ingenious algebraist.

#### THEOREM I.

If the cubic equation  $x^3 - px^2 + qx - r = 0$  has two equal roots, each of them will be  $(x) = \frac{pq - 9r}{2pp - 6q}$ .

#### DEMONSTRATION.

Call the three roots  $a$ ,  $a$ , and  $b$ ; then, by the composition of equations we shall have  $x^3 - 2a^2x^2 + 3aa^2x - aab = 0$ , where

$2a+b=p$ ,  $aa+2ab=q$ , and  $aab=r$ ; which values being written in our theorem, we have  $x \left( = \frac{pq-qr}{2pp-6q} \right) = \frac{2ada+4aab+2ab+2abb-9aab}{8aa+8ab+2bb-6aa-12ab} = \frac{2aaa-4aab+2abb}{2aa+4ab+2bb} = a$ . *E. D.*

## EXAMPLE I.

If the equation  $x^3 + 5x^2 - 32x + 36 = 0$  has two equal roots, it is proposed to find them by the above theorem.

Here  $p = -5$ ,  $q = -32$ , and  $r = -36$ ; these values being written in the theorem, we have  $\frac{-5 \times -32 - 9 \times -36}{2 \times 25 - 6 \times -32} = \frac{160 + 324}{50 + 192} = \frac{484}{242} = 2$ , which being written for  $x$ , the equation becomes  $8 + 20 - 64 + 36$ , which is evidently  $= 0$ ; consequently 2 and 2 are roots of it.

Otherwise, 2, the value of  $x$  given by the theorem, being written for it in the quadratic equation  $3x^2 + 10x - 32 = 0$ , the result is  $12 + 20 - 32 = 0$ .

Or, dividing the given cubic by the quadratic  $\overline{x^2 + 2x - 2}$ , we have  $x^3 - 4x + 4 \div x^3 + 5x^2 - 32x + 36 (x + 9$ ; therefore the three roots are 2, 2, and  $-9$ .

## EXAMPLE II.

Given  $x^3 + \frac{10}{7}x^2 - \frac{4000}{9261} = 0$ , an equation which has equal roots, to find them.

Here  $q = 0$ , and the theorem gives  $\frac{-36000 \times 49}{200 \times 9261} = \frac{-20}{21}$ , which value being written for  $x$  the equation vanishes.

THEOREM

THEOREM II.

If the biquadratic equation  $x^4 - px^3 + qx^2 - rx + s = 0$  has two equal roots, make  $A = \frac{12r - 2pq}{3pp - 8q}$ ,  $B = \frac{pr - 16s}{3pp - 8q}$ ,  $C = \frac{4B - 2q}{4A + 3p}$ , and  $D = \frac{r}{4A + 3p}$ , and you will have  $x = \frac{D - B}{A - C}$ .

A synthetical demonstration of this theorem would be very long: the INVESTIGATION is as follows.

It has been demonstrated by the writers on algebra, that, if a biquadratic equation, as  $x^4 - px^3 + qx^2 - rx + s = 0$ , has two equal roots, one of them may be had from the equation  $4x^3 - 3px^2 + 2qx - r = 0$ . Multiply this equation by  $x$ , and the original one by 4, and take the difference of the two, which will be  $px^3 - 2qx^2 + 3rx - 4s = 0$ . Again, if this equation be multiplied by 4, and the other cubic by  $p$ , and their difference taken, we shall have  $3pp - 8q \times x^2 + 12r - 2pq \times x + pr - 16s = 0$ , or  $x^2 + \frac{12r - 2pq}{3pp - 8q} x + \frac{pr - 16s}{3pp - 8q} = 0$ , or  $x^2 + Ax + B = 0$ , putting  $A$  and  $B$  for the known quantities in the second and third terms. Now multiply this equation by  $4x$ , and take the first cubic from it, and we shall have  $4A + 3p \times x^2 + 4B - 2q \times x + r = 0$ , which being divided by  $4A + 3p$ , and  $C$  and  $D$  put equal to  $\frac{4B - 2q}{4A + 3p}$  and  $\frac{r}{4A + 3p}$  respectively, gives  $x^2 + Cx + D = 0$ ; and this equation being taken from the other quadratic, there remains  $A - C \times x + B - D = 0$ ; consequently  $x = \frac{D - B}{A - C}$ . Q. E. I.

COROLLARY I. From the above investigation it appears, that one of the equal roots may also be obtained from either of these two quadratic equations, of which the first seems most eligible,



as the co-efficients of it are less complex than those of the other:

$$\overline{3pp - 8q} \times x^2 + \overline{12r - 2pq} \times x + pr - 16s = 0,$$

$$\text{and } \overline{4A + 3p} \times x^2 + \overline{4B - 2q} \times x + r = 0. \quad \text{And these,}$$

when  $p=0$ , become  $-8qx^2 + 12rx - 16s = 0$ ,

$$\text{and } -\frac{48r}{8q}x^2 + \frac{64s}{8q} - 2q \times x + r = 0,$$

$$\text{or } x^2 - \frac{3r}{2q}x + \frac{2s}{q} = 0,$$

$$\text{and } x^2 + \frac{qq - 4s}{3r}x - \frac{q}{6} = 0.$$

COROL. 2. If both  $p$  and  $q$  vanish, then, from either of the quadratics we get  $x = \frac{4s}{3r}$ , perfectly agreeing with the cubic  $px^3 - 2qx^2 + 3rx - 4s = 0$ , which, when  $p$  and  $q$  vanish, becomes  $3rx - 4s = 0$ . And this equation is of use; because, in this case, the theorem fails, one of the divisors being  $= 0$ .

COROL. 3. From the equation  $4x^3 - 3px^2 + 2qx - r = 0$ , which, when  $p$  and  $q$  vanish, becomes  $4x^3 - r = 0$ , we also get  $x = \sqrt[3]{\frac{r}{4}}$ , another expression of the same value of  $x$ .

COROL. 4. When  $r=0$ ,  $D=0$ , and from the equation  $x^3 + Cx + D = 0$ , we have  $x = -C$ .

#### EXAMPLE I.

If the equation  $x^4 - 9x^2 + 4x + 12 = 0$  has equal roots, it is proposed to find them.

Here

Here  $p=0$ ,  $q=-9$ ,  $r=-4$ , and  $s=12$ ; and

$$A \text{ becomes } = \frac{12 \times -4}{-8 \times -9} = \frac{-2}{3},$$

$$B \quad . \quad . \quad = \frac{-16 \times -12}{-8 \times -9} = \frac{-8}{3},$$

$$C \quad . \quad . \quad = \frac{4 \times \frac{-8}{3} + 18}{4 \times \frac{-2}{3}} = \frac{-11}{4},$$

$$D \quad . \quad . \quad = \frac{-4}{\frac{-8}{3}} = \frac{3}{2},$$

$$\text{and } \frac{D-B}{A-C} = \frac{\frac{3}{2} + \frac{8}{3}}{\frac{-2}{3} + \frac{11}{4}} = \frac{18+32}{-8+33} = \frac{50}{25} = 2,$$

which being written for  $x$ , the equation becomes  $16 - 36 + 8 + 12 = 0$ ; therefore 2 is one of the roots.

The same value of  $x$  may be discovered from either of the quadratic equations mentioned in corollary 1. The proper values of the co-efficients being written in the first of them, it becomes

$$x^2 - \frac{2}{3}x - \frac{8}{3} = 0, \text{ where one value of } x \text{ is } \frac{1+\sqrt{25}}{3} = 2. \text{ The}$$

other quadratic becomes  $x^2 - \frac{11}{4}x + \frac{3}{2} = 0$ , one of whose roots is

$$\frac{11+\sqrt{25}}{8} = 2.$$

#### EXAMPLE H.

It being known that the equation  $x^4 - x^3 - 7x^2 + 13x - 6 = 0$  has two equal roots, to find them.

Here  $p=1$ ,  $q=-7$ ,  $r=-13$ , and  $s=-6$ ; and  $A = \frac{-142}{59}$ ,  $B = \frac{83}{59}$ ,  $C = \frac{-1158}{391}$ ,  $D = \frac{767}{391}$ ,  $D-B = \frac{12800}{23069}$ ,  $A-C = \frac{12800}{23069}$ , and  $\frac{D-B}{A-C} = \frac{12800}{12800} = 1$ , one of the roots sought.

The

The same value of  $x$  may be found from either of the two general quadratic equations given in corollary 1. From the first of them we get one value of  $x = \frac{71 - \sqrt{144}}{59} = 1$ . And from the other, one value  $= \frac{579 - \sqrt{35344}}{391}$ , which is also  $= 1$ .

## EXAMPLE III.

Given the equation  $x^4 - \frac{1}{2}x + \frac{3}{16} = 0$ , in which two values of  $x$  are equal to each other, to find them.

By corollary 2. we have  $x = \frac{4 \times 3}{16} \div \frac{3 \times 1}{2} = \frac{8}{15} = \frac{1}{2}$ . By corol. 3.  
 $x$  is  $= \sqrt[3]{\frac{1}{8}} = \frac{1}{2}$ .

## THEOREM III.

If the furfold equation  $x^5 - px^4 + qx^3 - rx^2 + sx - t = 0$  has two roots equal to each other, and you make  $A = \frac{15r - 3pq}{4pp - 10q}$ ,  $B = \frac{2pr - 20s}{4pp - 10q}$ ,  $C = \frac{25t - ps}{4pp - 10q}$ ,  $D = \frac{5B - 3q}{5A + 4p}$ ,  $E = \frac{5C + 2r}{5A + 4p}$ ,  $F = \frac{s}{5A + 4p}$ ,  $G = \frac{B - E}{A - D}$ ,  $H = \frac{F + C}{A - D}$ ,  $I = \frac{B - H}{A - G}$ , and  $K = \frac{C}{A - G}$ , then shall one of the equal values of  $x$  be  $= \frac{H - K}{I - G}$ .

The investigation of this theorem being altogether similar to that of the last, it is unnecessary to give it here.

The difference of equations being taken as in the investigation of theorem II. it will appear, that one of the equal roots may also be had from any one of the following five equations, of which sometimes one, sometimes another, will be the most eligible.

1.  $5x^4 - 4px^3 + 3qx^2 - 2rx + s = 0.$

2.  $px^4 - 2qx^3 + 3rx^2 - 4sx + 5t = 0.$

3.  $x^3 + Ax^2 + Bx + C = 0.$

4.  $x^3 + Dx^2 + Ex - F = 0.$

5.  $x^3 + Gx + H = 0.$

It is obvious, that, when  $p$  vanishes, the work will be considerably shortened; and when both  $p$  and  $q$  are wanting, though the above *formula* fails, yet the equal root may be easily obtained from the equation  $px^4 - 2qx^3 + 3rx^2 - 4sx + 5t = 0$ , which in that case becomes  $3rx^2 - 4sx + 5t = 0$ . Whenever  $s$  is wanting,  $F$ , in the second cubic above, will be  $= 0$ , and consequently  $x$  may be found from the quadratic equation  $x^2 + Dx + E = 0$ . But in any of these cases the equal root may be found by division. However, the operation probably will not, in general, be so short as extracting the root of the quadratic; I will therefore hasten to give an example or two of the use of the theorem.

EXAMPLE I.

Given  $x^5 + x^3 - x^2 + 0.09433 = 0$ , to find  $x$ , two values of it being equal to each other.

Here  $p = 0$ ,  $q = 1$ ,  $r = 1$ ,  $s = 0$ ,  $t = -0.09433$ , and we get

$A = -1.5$

$F = 0$

$B = 0$

$G = -0.2231$

$C = +0.2358$

$H = -0.1241$

$D = +0.4$

$I = -0.0972$

$E = -0.4238$

$K = -0.185$

and  $x = \frac{H-K}{1-G} = 0.48.$

The proper values of the co-efficients being written in the five equations before mentioned, and some of them divided by the

the co-efficient of the highest power of  $x$ , we have these four equations, in each of which one value of  $x$  is one of the equal ones sought :

$$x^3 + 0.6x - 0.4 = 0.$$

$$x^3 - 1.5x^2 + 0.2358 = 0.$$

$$x^2 + 0.4x - 0.4238 = 0.$$

$$x^2 - 0.2231x - 0.1241 = 0.$$

Now the most eligible equation is the quadratic  $x^2 + 0.4x - 0.4238 = 0$ , whose affirmative root is  $\sqrt{0.4638} - 0.2 = 0.4811$ , agreeing with the value of  $x$  found above, but true to two places lower in the decimal.

#### EXAMPLE II.

To find the two equal values of  $x$  in the equation  $64x^3 - 20x^2 + 3 = 0$ .

The given equation being divided by 64, we have  $x^3 - 0.3125x^2 + 0.046875 = 0$ ; and then, from the first of the five equations given above, we get  $5x^2 - 0.625x = 0$ , and  $x = \sqrt[3]{0.125} = 0.5$ . But from the second of the equations just mentioned, we have  $0.9375x^2 - 0.234375 = 0$ , or  $x^2 = \frac{0.234375}{0.9375} = 0.25$ , and  $x = \sqrt{0.25} = 0.5$ .

From the foregoing few pages it is evident, that rules may be made for finding the equal roots of equations of more than five dimensions by division; but the operations by them will, in most cases, be long and tedious. It is obvious, however, that such equations may be depressed to any dimension the algebraist pleases.

It has indeed been supposed, that the number of equations that have equal roots is but small, and, consequently, that the  
chief

chief use of the rules for finding their roots is to get limits and approximations to the roots of equations in general. That use, it must be allowed, were it the only one, is sufficient to pay for investigating them. But if the equations that have equal roots should hereafter be found not so few as has been generally received, then the use of the above theorems will become more extensive.

I beg leave to add, that this short essay is but a small part of a work, in which, if I should ever have leisure to put a finishing hand to it, something more on this subject may very probably appear. In the mean while, I hope, this little piece will be candidly received by those who have more leisure and better abilities for studies of this kind.

Constantine,  
February 9, 1782.

XXVII. *Some farther Considerations on the Influence of the Vegetable Kingdom on the Animal Creation.* By John Ingen-houfz, Counsellor to the Court, and Body Physician to the Emperor, F. R. S. &c.

Read June 13, 1782.

UPON being informed, a few months ago, as well by private letters as from the Critical Review, that my doctrine was quite over-turned by the fifth volume of Dr. PRIESTLEY, and by an experiment quoted in the book of Mr. CAVALLO on Air; I invited some of my friends here to assist at some decisive experiments, of which I will here give an exact account. I told them the whole result which was to be expected from them, if my system was founded on nature, explaining to them before-hand the theory of these results, and promising, at the same time, that, if the result should fail, I should myself be the first to discredit my own system. I had the satisfaction to convince them that the result did fully answer my prediction and expectation. These experiments are the following, all made in a hot-house of the Botanical Garden in the winter of 1782.

I exposed to the sun-shine six globular glass vessels, each containing about 160 cubic inches of space, all filled with pump-water, which was boiled during more than two hours, and poured quite hot into the glass vessels, on purpose to prevent any access of air to the water.

Exp. I. In two of these vessels I put as much of the *conserva rivularis* (a water plant, classed by LINNÆUS amongst the *cryptogamia*) as was sufficient to take up the space of about an inch square.

Exp. II. In the two next vessels I suspended by threads tied to bits of cork, some pieces of woollen and silk cloth of different colours, as white, scarlet, green, and brown, having previously wetted them in some boiled water, on purpose to free them from all air.

Exp. III. In the two remaining vessels I placed nothing at all.

Exp. IV. In another vessel of the same form and size I put some of the *conserva rivularis*, and filled it with pump water.

All these globular vessels were inverted, with their orifices immersed in vessels filled with quicksilver, for the purpose of preventing effectually any communication between the contents of the vessels and the atmosphere.

*Result of experiment I.* The first two days neither of the vessels contained any air, and even the small quantity of air, which here and there adhered in the form of a bubble to the fibres of the vegetable when it was shut up in the vessel, had entirely disappeared. The third day, in the morning, some air bubbles began to rise from every part of the *conserva* in both glasses; and in the afternoon of the same day, a great quantity of air bubbles rose continually from it. I took at that time the vegetable out of one of these vessels. I plunged a wax taper, just extinguished, into the orifice of this vessel, on purpose to see whether the air, already extricated from the *conserva*, was dephlogisticated or not. The wax taper took flame immediately with an uncommon splendour. After this I poured the half of the water from the globular vessel into a common bottle, and corked it. I inverted this bottle afterwards



in an earthen vessel filled with boiled water. I placed this apparatus near the fire till the water in the bottle began to boil; after which I cooled the whole, and found a good quantity of air collected in the bottle, which air proved to be dephlogisticated. When I drew the vegetable out of the glass vessel I observed the water to sparkle almost like Seltzer water, or like water impregnated by art with fixed air. The vegetable which was still kept in the second bottle of exp. I. continued to yield air in the sun-shine, till it ceased to throw up any more air, towards the seventh or eighth day of its being shut up in the vessels. When, after this time, this globular vessel was shook, the water became full of small air bubbles, which for the most part rose to the inverted bottom of the vessel, great part of them settling upon the vegetable, which appeared all covered with them. This sparkling air, which became visible by shaking the glass, could not but be air originally produced by the *conserva*, and so loosely joined with the water, that it disengaged itself in a great measure from it by the motion of the vessel. After the tenth day the vegetable began to appear withered, grew yellow, and began to die. I found about eight cubic inches of dephlogisticated air collected in the vessel. This proved to be of a very eminent quality, its goodness being of  $352^{\circ}$ ; that is to say, that from a mixture of one measure of this air, and as many measures of nitrous air as were necessary to complete the full saturation, there were destroyed three measures and  $\frac{2}{3}$  of a measure, the test being made with Abbé FONTANA's Eudiometer, employed in the manner described in my book upon Vegetables, p. 278. *et seq.* The quality of this air was superior to that of any air I ever got from this plant in fresh pump water, its goodness proving, in general, to be from  $260$  to  $330^{\circ}$ , in the hot-house; this was during the winter, for I never had been

been able to obtain such fine air from this vegetable in the summer\*; the reason of which I will explain elsewhere.

*Theory of exp. I.* Boiled water, having lost its air, is very much disposed to imbibe it from all bodies which contain this fluid; and therefore, during the first day or two of its exposure to the sun-shine with the vegetable, this water absorbed all the air which the plant emitted; and even that which had remained entangled between the fibres of the vegetable when it was immersed in this water. The water being at last saturated with this air could take up no more; and therefore, whatever air, after this saturation, came forth from the vegetable rose to the top of the vessel. The quantity of this dephlogisticated air was smaller than that which an equal bulk of the same plant commonly yields in fresh pump water, because a great deal of air was at first taken in or absorbed by the boiled water; which absorption does not happen, or at least is not so great,

\* By continuing to make experiments, during the whole winter, in the hot-houses of the botanical garden, I found that the *conserua ricularis* yielded dephlogisticated air of a much superior quality to that I had ever been able to get from it in the summer, in the open air; whereas those plants, such as the *agave Americana*, *cactus triangularis*, &c. which yielded in the summer the best air, did scarce yield any in the winter (and that of a quality scarce better than common air) though placed next to the *conserua*. The quantity of dephlogisticated air I got in the winter from the *conserua* was so great, that as much of this vegetable as occupies about the space of one cubic inch commonly yielded from 12 to 16 cubic inches of this air in the space of three or four days, when the sun did shine, the quantity of pump-water being about 160 cubic inches. The green matter which Dr. PRIESTLEY mentions as spontaneously produced from pump-water, gave in the winter also a tolerable quantity of dephlogisticated air, of a good quality, though not so fine, or in such large quantities, as it is used to give in the summer. It seems to be a general rule, that the greater the quantity of air obtained from vegetables in the sun is, the better is its degree of goodness.

when

when pump water is used, as this water is always nearly saturated with air. The air, thus obtained in boiled water, was of a finer quality than that commonly obtained by the same means in pump water; because this air, being entirely free of air disengaged from fresh water, must of course be so much the purer dephlogisticated air. The water of the first vessel, and which was taken out of it as soon as air bubbles began to rise from the plant, sparkled like Seltzer water, and yielded by heat dephlogisticated air, because it was then already saturated with dephlogisticated air issuing from the vegetable. The water of this vessel being shook, after the vegetable ceased to throw up any more air in a visible way, still continued to sparkle; because, though the vegetable by losing gradually its vigour, was at last no more able to throw up air in visible bubbles, yet it had still enough of its vital power left to keep the water saturated with dephlogisticated air, so as to sparkle when shook. This vegetable, continually robbing the water of its natural air, found at last nothing more in it to support its life, and therefore at last languished and perished; which it did so much the sooner, from the contact of the dephlogisticated air, with which the water was impregnated, and to which the vegetable had been all that time exposed, having hurt its constitution (it is well known, that plants die in dephlogisticated air) and thus hastened its death. The sparkling quality of the water did not cease entirely till the vegetable was quite deprived of its life. The water began to sparkle every day very briskly, by being shook after the apparatus had been exposed an hour or two to the sun-shine, during the time the vegetable was in its full vigour, and ceased to do so some time after sun-set, or after the apparatus had been withdrawn from the sun's light; because this vegetable, like all others, elaborates no dephlogisticated air but by the

influence of the sun; and because this air, being but loosely united with the water, disengaged itself again from it, and rose to the inverted bottom of the vessel. The water being thus deprived of the dephlogisticated air, ceased to sparkle any more, till it became again saturated with it, after the apparatus had been exposed again, during some time, to the influence of the light.

*Result of exp. II.* No air at all was produced in the vessel containing the pieces of cloth, during three weeks exposure to the sun-shine.

*Theory of exp. II.* Boiled water, having lost its air, could yield none, at least till after a long time, when some degree of corruption took place in the animal substance, viz. the pieces of cloth.

*Result of exp. III.* Not an atom of air has appeared in this vessel, though it stood about two months upon the same place.

*Theory of exp. III.* Boiled water having no air, the sun could extricate none from it.

*Result of exp. IV.* the *conserva* began to yield air bubbles the very same day, a little while after its exposure to the sun. The next day it threw up an immense quantity of them. The fifth day it began to throw up less, and ceased entirely about the seventh day, when the quantity of about fourteen cubic inches of dephlogisticated air, of an excellent quality, though less fine than that obtained in exp. I. was collected. The water sparkled, as does Seltzer water, by the vessel being shook. This water being exposed to the fire, in an inverted vessel, yielded a good quantity of air, which was so far dephlogisticated as to be able to kindle a wax taper just extinguished. After the seventh day the vegetable began to die.

*Theory*

*Theory of exp. IV.* The vegetable threw up very soon air bubbles, because this water, being in its natural state, and thus saturated with air, could not absorb much of the air issuing from the vegetable, which air must, of course, soon rise up in visible bubbles. A great deal more air was collected than in exp. I. because less of the air issuing from the plant was absorbed by this water than in exp. I. The air obtained was not so good as that obtained in exp. I.; because the air in this experiment was somewhat infected by the air issuing from the water, which was but common air. The water sparkled when the vessel was shook, because this water, though it had probably lost some of its own air, yet had assumed a great deal of air from the plant \*, which air disengages itself from the water very easily, just as fixed air does; the more so when the water is moved.

\* It seems, that dephlogisticated air has by no means such a strong attraction to water, in other words, has not so much affinity with it, as common air has: it appears to be but loosely united with water, and quits it very easily by the water being shaken, or even though the water be kept quiet. May not this quality be looked upon as a providential one? for thus the dephlogisticated air, produced by water plants, is continually emitted by the water, and diffused through the common stock; and for this reason the water, when shook during the day-time, always sparkled as champagne; because at that time the water was always kept saturated by the dephlogisticated air, issuing continually out of the vegetable. But the water ceased to sparkle, after having been withdrawn from the sun-shine during some hours; because the dephlogisticated air, with which the water was saturated during the day-time (supposing the apparatus to be kept exposed to the sun) being but loosely united with the water, disengaged itself gradually from it, and rose to the top of the vessel. The sparkling quality of this water returned after the vessel had been exposed afresh to the sun; because the vegetable, resuming in that exposition its daily operation, communicated a fresh supply of dephlogisticated air to the water. This sparkling quality returned every day, as long as there remained any life in the vegetable; after which the water sparkled no more (though shook ever so much) either by day or in the night.

This water yielded, by heat, true dephlogisticated air; whereas the same water, when it has not been exposed to the action of a vegetable, yields by heat nothing but common air. The reason of it is, that the air elaborated by the plant, with which this water was saturated, was real dephlogisticated air. The vegetable at last languished and began to die, because the water was impregnated with dephlogisticated air, which being an excrement of the plant is hurtful to its constitution. Besides, this water had at last lost the most part of, or perhaps all, its own stock of common air; and with this all the nutritive nourishing and phlogistic particles, which were taken in by the plant, and was therefore become less fit to keep up vegetable life.

All these experiments were repeated frequently, and always with the same general results\*.

I think the abovementioned facts will be looked upon as quite sufficient to put my doctrine out of all farther question. I have many more facts, perhaps equally demonstrative with those just described; but, as this paper is already too long, I will keep them for some other opportunity. However, I cannot forbear making some farther remarks by which the point in question may be still farther illustrated.

If it was the water, and not the vegetable, which yielded the dephlogisticated air; and thus, if the reason why water plants and the green matter cease at last to throw up more air (if the

\* That air is thrown out of living vegetables exposed to the sun-shine, was already observed by the rev. Dr. HALES, as may be seen in vol. I. of his Statical Essays, p. 110. The apparatus which he used for this experiment is represented by fig. XVII. plate VII. But this inestimable philosopher, not even suspecting that this air was of a peculiar nature, did not collect it. BOYLE obtained much air from vegetables *in vacuo*.

water is not renewed) is, that the water being at last exhausted of its air can yield no more, it would necessarily follow, that the water, thus supposed to be deprived of air, should be similar to distilled or boiled water; but it is quite the reverse. This very water, instead of being exhausted of air, gives evident signs of being over saturated with it. It sparkles almost as champaign does, when shook by day-time in the sun; and it will, when it is at that time separated from the vegetable, yield by itself in the sun, but more so by the fire, a good quantity of dephlogisticated air. The reason why the green matter ceases at last to yield more air is therefore by no means because the water is exhausted of air; but, on the contrary, because it is too much saturated with it, and that with an air hurtful to vegetable life, and because this water has at last lost its own natural air, and together with this air the nourishing and phlogistic particles which are necessary to keep up the full vigour of plants.

If it was the water, and not the vegetable, which furnished the dephlogisticated air, why should the air bubbles not settle indifferently on either surface of the leaves? In this supposition, how could that admirable regularity be accounted for, by which all the leaves of a vine and a lime-tree are first covered with air bubbles on the under side, and all the leaves of *lauro-cerasus*, at the upper surface, whichever surface of the leaves is exposed to the rays of the sun? Why should the air always settle on most leaves in the form of bubbles, but never so upon the leaves of the *trapaolum majus*, at least during the first hours, but always in the form of bags, adhering to the upper edge of the leaves, and detaching themselves when they are grown to a certain size,

size, which never happens with leaves of a vine or lime-tree\*; whereas the green stalks of this plant (the *tropæolum majus*) are at the same time all covered with separate air bubbles? More of these remarkable appearances in different plants are to be seen in the third section of my book on Vegetables.

If the dephlogisticated air, obtained by means of vegetables in water, was air deposited from the water, and purified of its phlogiston, by remaining, during a certain time, in contact with the vegetable, it would follow, that this air would be so much the purer the longer it remains in contact with the vegetable; but it is quite the reverse. The air obtained from the leaves of a vine, to which the air bubbles stick a long while before they detach themselves and rise up, is never by far so much dephlogisticated as is the air obtained from some American plants, out of which the air rushes almost all in continual streams, as so many springs, not remaining a single moment upon the leaves. The difference of both airs is so great, that I never got dephlogisticated air from leaves of a vine, lime-tree, and such like, whose goodness surpassed  $260^{\circ}$  (it is commonly a little above  $200^{\circ}$ ); whereas I got commonly, from the above mentioned American plants, an air whose goodness was of above  $300^{\circ}$ , sometimes even of above  $350^{\circ}$ , in a very fair day, this air being put to the test, according to the manner which I have described before†.

If

\* I have explained the reason of this singularity in that plant, in my book, where it is mentioned by the name of *nasturtium indicum*.

† The *sempervivum tetlorum*, which grows almost every where upon the roofs of houses, gives also a very great quantity of dephlogisticated air of an eminent quality. It should seem, that all fleshy plants particularly excel in the quality of yielding fine dephlogisticated air, and a great quantity of it. The *agave Ame-*



If it was the water, and not the plant, which yielded the dephlogisticated air, the quantity of air obtained would bear, in general, a proportion to the quantity of the water employed; but this is by no means the case. The quantity of air bears a proportion to the bulk of the vegetable much more than to the quantity of the water. This is very easily to be observed with some of the above mentioned American plants. It ought to be always observed, that if too many leaves are crowded together, they shade one another too much; and therefore, in this case, the quantity of air obtained will be proportionably less, and its quality worse.

Again: if the dephlogisticated air, obtained from plants in water, was air disengaged from the water, it would follow, that a plant shut up in a transparent glass vessel without any water would yield no air at all, nor increase the quantity of air shut up with the vegetable. The following experiment, I think, will be sufficient to convince any one that this is far from being the case. I placed in a glass tube, hermetically sealed at one end, a piece of an American plant, called *cereus*; the extremity of this piece, where it was cut from the plant, was tightly squeezed in a small glass vessel, in which only as much water was kept as seemed to be required to keep the *cereus* in full vigour. I smeared the vegetable, and the orifice of the glass vessel all around with soft wax, so that all communication between the air within the tube and the water within the small vessel was cut off. I placed this tube inverted in a vessel filled with quicksilver, keeping a column of some inches of

*ricana* gives such a prodigious quantity of dephlogisticated air, that, in a fair day, I often got from one single leaf above 150 cubic inches of this air, of the finest quality. The *cactus triangularis*, *cereus*, *sempervivum arborum*, and many others give no less air.

the metallic fluid in the tube, to allow the air within the tube to expand by the heat of the sun without escaping. After this apparatus had stood during a few hours, in a bright sun-shine, I cooled the whole to the same degree it possessed when it was exposed to the sun. This was done by plunging the whole in a tub filled with water, whose degree of heat was restored to the same degree it had before. I found the quantity of air within the tube remarkably increased, and so far dephlogistified that a flame burned in it with an increased brightness, and that one measure of it joined to one measure of nitrous air occupied 0.64; whereas the same air, before it was shut up with the *cereus* was of such a degree of goodness, that one measure of it with one of nitrous occupied 1.06. Though this experiment may fully shew that plants throw out air in the sun-shine, yet if another plant, which does not by nature yield so much air as the *cereus* commonly does, is shut up in a similar tube, and exposed to the sun, the same effect will not always be obtained. The reason of it is, that plants absorb a good quantity of common air as their nourishment at the same time that they throw out dephlogistified air. This fact, therefore, if considered by itself, will not be looked upon as equally demonstrative with the above mentioned ones. The result of this experiment may depend on the more or less vigour of the vegetable employed, on the more or less brightness of the sun's light, on the more or less heat the vegetable receives, &c. A *cereus* being a plant of hot climates may bear more heat than an European plant. All these circumstances, as well as many others, may make the issue of this experiment sometimes ambiguous; but the fact, as I have related it, joined to the above mentioned analogous experiments of

of Dr. HALES and Mr. BOYLE, will add strength to my assertion, viz. that vegetables really throw out air in the sun-shine.

If all what I have said hitherto should not be thought sufficient to take away the prejudice which Dr. PRIESTLEY's fifth volume, and Mr. CAVALLO's book on Air, may have produced in the mind of some philosophers, I should advise them to be present, at least once, at the most beautiful scene which they will behold, when a leaf of an *agave Americana*, cut in two or three pieces, is immersed in a glass bell or jar full of pump-water, inverted and exposed to the sun in a very fair day in the middle of the summer, when this plant is in its full vigour; and when they shall have seen those beautiful and continual streams of air, which rush from several parts of this vegetable, principally from the white internal substance of it, I will be answerable for their laying aside all farther doubt about the truth of my doctrine.

After having now demonstrated, as I think, in the clearest manner, that vegetables diffuse through our atmosphere, in the sun-shine, a continual shower of this beneficial, this truly vital air; and that plants immersed in water, far from robbing it of all air, impregnate it fully with a better and more salubrious air; let us not pass so wonderful, and hitherto not even so much as suspected, an operation of nature, without admiring the designs of that infinite wisdom, who has employed such hidden, such wonderful, and at the same time such beneficial means to preserve from destruction the living beings which inhabit our earth; and let us consider, whether it would not be worth while to attempt drawing some benefit from this new discovery, by making use of vessels of water, in which some leaves of vegetables have been exposed in the sun-shine; by placing such vessels in our rooms; by stirring the water; by  
 2 sprinkling

- A. a globular glass vessel containing about 160 cubic inches of water.
- B. glass vessel fill'd with Mercury in which the orifice of the spherical vessel is plunged.
- C. a vegetable called *Conferva rivularis*.
- D. a piece of wood to which the *Conferva rivularis* is attached to keep it in its place.



sprinkling with it our floors instead of using for this purpose common water; by placing within our houses, instead of flower-pots, dishes containing some *conserva rivularis*, a plant to be met with almost every where; shooting forth with the utmost luxuriance in all water basons, in all tubs and vessels in which water is kept. Is it possible, after all this, not to believe, that the Creator has multiplied this vegetable with a similar view to our benefit? This benefit we may now, with some confidence, apply to our preservation, by honouring this vegetable with a place in those of our own rooms which are exposed to the sun, and keeping it alive as long as we please; which may be done by only pouring every day fresh water upon it, and squeezing gently now and then out of it the dephlogisticated air with which the whole mass swells up almost as soon as the sun casts its rays upon it. The water itself, in which it has been immersed, will now, perhaps, be looked upon as too precious to be thrown away as useless and deprived of that very principal of animal life, of which I have demonstrated it to be highly pregnant.

XXVIII. *A Microscopic Description of the Eyes of the Monoculus Polyphemus LINNÆI.* By Mr. William André, Surgeon; communicated by Sir Joseph Banks, Bart. P. R. S.

Read May 30, 1782.

THE wonderful structure of the eyes of insects in general, most commonly illustrated by that of the *Libellula*, or *Dragon-fly*, cannot fail of striking with astonishment the naturalist who investigates the works of the great Creator in his most minute productions. According to LEWENHOEK, HOOK, and others, the *corneæ* of most insects are made up of an infinite number of small, transparent, horny lenses, each resembling, in some degree, a small magnifying glass. This structure prevails in the *corneæ* of insects in general; but the *Monoculus Polyphemus*, or *King Crab*, is, among others, an exception to this rule.

The *Monoculus Polyphemus*, or *King Crab*, is a crustaceous animal found in all the seas surrounding the continent of America and the West India islands, and which frequently grows to a very large size \*. I shall describe so much of the *Monoculus* only as is necessary to point out the situation of the eyes, which have been looked upon as two in number only †, though in reality they are four. The largest piece of the crustaceous covering of this animal, when separated from the rest of the shell, has very much the shape of a barber's basin, or the fore-

\* BOSSU's Travels, vol. I. p. 368.

† LINNÆI Systema Naturæ, tom. I. p. 1057.

part

part of a woman's bonnet. The eyes are a part of the shell, or, as LINNÆUS expresses it, they are *testæ innati* \*. They may be distinguished by the terms large and small, or lateral and anterior. If the shell were divided fairly in half, the large eyes would be nearly in the center of each piece, and the small ones on the divided edge near the fore-part of the shell. The large eyes are at a great distance from each other; but the small ones are close together. It will appear hereafter, that the large eyes are made up of a great number of small, transparent, amber-like cones, and that the small ones are composed of one such cone only; so that they may be divided into eyes with many cones, and eyes with a single cone †. The large eyes, or those with many cones, appear as two transparent spots about the size and nearly of the shape of a kidney bean, the concave edges looking towards each other, and the convex towards the edge of the shell. If they be examined attentively, we may discern on their surface a number of small depressions, which point out the center of each cone. The small eyes, or those with a single cone, look like two small transparent spots, not larger than a pin's head; these, from their minuteness, are easily overlooked, see fig. 1. where A.A. shew the large eyes, and B.B. the small ones.

The appearances which I have described may be seen on the external surface of the shell with the naked eye; but in order to proceed to a further investigation of the subject, the *corneæ*

\* This being the case, the eyes can enjoy no motion; in which particular, as well as in some others, the *Monoculus Polyphemus* differs from the genus of crabs, whose eyes are placed on petioles, or stalks, and are moveable.

† The Greek words *πολυς κωνος*, and *μονος κωνος*, would express the sense in a more concise manner. *Oculi polyconici et oculi monoconici*.



must be removed from the shell, and applied to a single microscope with a very strong light.

The internal surface of the large eyes, examined with the microscope, is found to be thick set with a great number of small, transparent cones, of an amber colour \*, the bases of which stand downward, and their points upwards next the eye of the observer †. The cones in general have an oblique direction, except some in the middle of the *cornea*, about thirty in number, the direction of which is perpendicular. The center of every cone being the most transparent part, and that through which the light passes; on that account the perpendicular or central cones always appear beautifully illuminated at their points. In a word, they are all so disposed as that a certain number of them receive the light from whatever point it may issue, and transmit it to the immediate organ of sight, which we may reasonably suppose is placed underneath them; but this last circumstance can only be determined in a recent subject, which I have never been so lucky as to see. The cones are not all of the same length; those on the edges of the *cornea* are the longest, from whence they gradually diminish as they approach the center, where they are not above half the length of those on the edges, see fig. 2.

As these cones so easily transmit the light through their substance, when I first examined them I thought they were tubes; but I have since viewed them broken in different directions, and am convinced they are solid transparent bodies. If they be viewed with a deep magnifier, every cone appears divided transversely by two or three internal *septa* or partitions.

\* I have made some attempts to ascertain their number, and think they amount to about 1000.

† This must be reversed if the eye be considered in its natural position.

This appearance is owing to the cones themselves being made up of several cones, one within another, the *septa* or partitions being nothing more than the *apices* or points of the external cones; but this will be further explained by considering that the *cornea* of the *Monoculus* may be divided into layers, the number of which, however, I cannot ascertain; but I once met with a *cornea* in which the internal layer and its cones was separated from the external *lamina* and their cones. A portion of the internal layer is shewn fig. 4.; and the cones, very much magnified, with their *septa* or partitions, are exhibited fig. 5.

It is very well known, that all crustaceous animals deposit their shells once a year, and are left with a soft, tender covering, which, after some time, acquires the hardness of the former shell. As the *cornea* in these animals is a part of the shell, it is reasonable to suppose, that the internal layer is left with the soft covering, containing the rudiments of the future *cornea*; and this is the more probable, from what I have before observed, that I have met with an eye where the internal layer was separated from the more external ones, see fig. 4.

The structure of the small eyes being less elaborate than that of the large ones, their internal appearance, when placed in the microscope, will be described in a few words. They consist of an oval, transparent, horny plate, of an amber colour, in the center of which stands a single cone, through which and the oval plate the light passes, see fig. 3\*.

Having thus described, as concisely as possible, the singular mechanism of the *corneæ* of the *Monoculus*, I shall add a few words concerning their use. The lenticular structure of the *corneæ* of insects in general certainly assists in condensing or

\* The small eyes are analogous to those small eyes of other insects which entomologists have called *stemmata*.

strengthening the light in its passage to the immediate organ of sight. It is probable, that the cones in the *Monoculus* have the same effect. Whether they answer that purpose, in a more or less perfect manner than the lenses in the generality of insects, is what I cannot take upon me to determine.

EXPLANATION OF THE PLATE.

Fig. 1. The *Monoculus Polyphemus*.

AA. The large eyes.

BB. The small ones.

Fig. 2. One of the large eyes magnified.

Fig. 3. One of the small eyes magnified.

Fig. 4. A portion of the internal layer magnified.

Fig. 5. The cones magnified with their *septa* or partitions.





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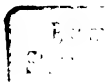
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# ERRATA.

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72. 2. *for* communicated *read* communicated
77. 12. *for* mercury removed without causing this to rise *read* removed  
without causing the mercury to rise
81. 9. *for* smaller towards the middle *read* larger towards the middle
391. — the last line, *for* — *read* +
403. 12. *for* — 54 *read* — 46
408. 3. from the bottom, *for*  $\frac{rn+m}{n}$  *read*  $\frac{n}{rn+m}$

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